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TRANSACTIONS

OF THE

OYAL ACADEMY OF MEDICINE IN IRELAND.

TRANSACTIONS

OF THE

Royal Academy of Medicine

IN IRELAND.

VOL. XXVIII.

EDITED BY

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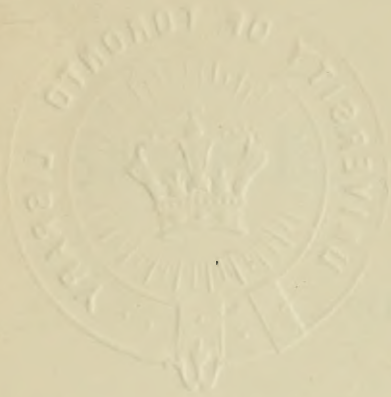

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Printed by John Falconer, Dublin.

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1890 Jellett, Henry, M.D., F.R.C.P.I., King's Professor of Midwifery, School of Physic, Trinity College, 34 Merrion-square. [1]

1897 Johnston, G. Jameson, M.B., R.U.I., F.R.C.S., Surgeon Royal City of Dublin Hospital, 13 Lower Fitzwilliam-street.

1905 Johnston, Henry M., M.B., B.Ch., Univ. Dub., Chief Demonstrator of Anatomy T.C.D., School of Anatomy, Trinity College, Dublin.

1893 Joynt, Richard Lane, M.D., Univ. Dub., F.R.C.S., Surgeon Meath Hospital, 84 Harcourt-street, Dublin. [1]

1905 Keegan, John Leo, F.R.C.S., Surgeon Jervis-street Hospital, 56 Lower Baggot-street, Dublin.

1899 Kennedy Denis, F.R.C.S., Surgeon Jervis-street Hospital and Children's Hospital, Temple-street, 68 Merrion-square.

1884 Kidd, Fred. W., M.D., Professor of Midwifery, R.C.S., ex-Master Coombe Hospital, Gynæcologist Meath Hospital, 17 Lower Fitzwilliam-street, Dublin. [1]

† Kinkead, Richard John, M.D., L.R.C.S., Lecturer on Medical Jurisprudence, Professor of Obstetric Medicine Queen's College, Galway, Foster House, Galway.

1899 Kirkpatrick, T. Percy C., M.D., Dub., F.R.C.P.I., Physician to Steevens' Hospital, 23 Lower Baggot-street. [1]

1901 Law, S. Horace, M.D., F.R.C.S., Throat Surgeon Adelaide Hospital, 49 Merrion-square.

1884 Ledwich, Edward L'E., L.R.C.P. & S.I., Physician to Mercer's Hospital, 38 Lower Leeson-street, Dublin.

1900 Leeper, Richard R., F.R.C.S., St. Patrick's Hospital, Dublin.

1891 Lennon, Edward Emmanuel, F.R.C.P., Physician Meath Hospital, 22 Merrion-square, Dublin.

† Lentaigne, John, P.R.C.S., Surgeon Mater Misericordiæ Hospital 42 Merrion-street, Dublin.

1897 Letters, Patrick, M.D., Valentia Island.

1898 Lindsay, James A., M.D., F.R.C.P. London, Professor of Practice of Medicine Q.U.B., Physician Royal Victoria Hospital, Belfast, 3 Queen's Elms, University-road, Belfast.

† Little, James, M.D., Ex-President R.C.P., Regius Professor of Physic Univ. Dub., 14 St. Stephen's-green, North, Dublin.

1897 Lumsden, John, M.D., Physician to Mercer's Hospital, 4 Fitzwilliam-place.

† M'Ardle, John Stephen, Professor of Surgery, University College, Dublin, F.R.C.S., Surgeon St. Vincent's Hospital and the Mullen Convalescent Home, 72 Merrion-square, Dublin.

1887 M'Causland, Richard Bolton, F.R.C.S., Surgeon Steevens' Hospital, 79 Merrion-square, Dublin.

1910 M'Connell, Adams A., M.B., 31 Lower Baggot-street.

1904 MacGrath, James Joseph, L.R.C.P., St. Helens, Dunfanaghy, Co. Donegal.

1895 M'Hugh, Michael, M.A., M.D., Univ. Dub., L.R.C.S., Professor of Materia Medica R.C.S., Physician St. Vincent's Hospital, 25 Harcourt-street, Dublin.

1902 M'Kisack, H. L., M.D., Physician Royal Victoria Hospital, 5 Chlorine-place, University-road, Belfast.

1905 M'Loughlin, E. P., M.D., R.U.I., Professor of Anatomy Catholic University Medical School, Cecilia-street, Dublin.

1903 McVittie, R. B., M.D., 62 Fitzwilliam-square.

1887 McWeeney, E. J., M.D., F.R.C.P.I., Professor of Pathology, University College, Dublin, 84 St. Stephen's-green, Dublin. [1].

1900 Maguire, Katharine M. N., M.D., 67 Merrion-square, South.

1883 Marques, L. Pereira, M.R.C.P., L.R.C.S., late Medical Officer Victoria Gaol Hospital, Macao, China.

1904 Matson, John A., M.D., Univ. Dub., F.R.C.P.I., Assist. Phys. Richmond, Whitworth, and Hardwicke Hospitals, 16 North Great George's-street.

1897 Maunsell, R. Charles B., M.B., B.Ch., B.A.O., Dub., F.R.C.S., Surgeon to Mercer's Hospital, 32 Lower Baggot-street.

1902 Meldon, G. Pugin, M.D., F.R.C.S., Surgeon Westmoreland Lock Hospital, 68 Lower Baggot-street.

1904 Mills, John, M.B., B.S., R.U.I., District Asylum, Ballinasloe.

1907 Milroy, T. H., M.D. Edin., F.R.S.E., Dunville Professor of Physiology, Queen's University, Belfast, Thornlea, Malone Park, Belfast. [1]

1901 Mitchell, A. B., F.R.C.S., Surgeon Royal Victoria Hospital, 4 College-square, Belfast.

1894 Montgomery, Robert John, M.A., M.B. Univ. Dub., F.R.C.S., Ophthalmic Surgeon Drumcondra Hospital, Assistant Surgeon Royal Victoria Eye and Ear Hospital, 28 Upper Fitzwilliam-street, Dublin.

1897 Mooney, H. C., M.B., F.R.C.S., Ophthalmic Surgeon Children's Hospital, Temple-street ; Assistant Surgeon Royal Victoria Eye and Ear Hospital, 22 Lower Baggot-street.

1894 Moore, Henry, L.R.C.P. & S., Surgeon Royal City of Dublin Hospital, 19 Upper Fitzwilliam-street.

† Moore, Sir John William, M.D., D.Sc. Oxon., Ex-President R.C.P., Physician Meath Hospital, Professor of Practice of Medicine Royal College of Surgeons, 40 Fitzwilliam-square, West, Dublin.

1904 Moorhead, T. Gillman, M.D., F.R.C.P., Physician City of Dublin Hospital, 23 Upper Fitzwilliam-street. [1]

1883 Murphy, John, F.R.C.P., Physician Mater Misericordiæ Hospital, 13 Merrion-square, Dublin.

1904 Murphy, W. L., M.A., M.B., Cantab., F.R.C.S.I., Surgeon, Nose and Throat St. Vincent's Hospital, 33 Upper Merrion-street.

1886 Myles, Sir T., Ex-President R.C.S., Surgeon Richmond Hospital, 33 Merrion-square, Dublin.

1908 Neill, Thomas, M.B., B.Ch., Dub.; Ex-Assistant. Master Coombe Hospital, 69 Harcourt-street.

† Nixon, Sir Christopher, Bart., M.D., Ex-President R.C.P., Physician to Mater Misericordiæ Hospital, 2 Merrion-square, N., Dublin.

1889 Nolan, Michael James, L.R.C.P., L.R.C.S., Resident Medical Superintendent Down District Lunatic Asylum, Downpatrick.

1899 O'Brien, C. M., M.D., L.R.C.P. & S., Physician to City Hospital for Diseases of the Skin, 29 Merrion-square.

† O'Carroll, Joseph Francis, M.D., F.R.C.P., Physician Richmond, Whitworth, and Hardwicke Hospitals, 43 Merrion-square, Dublin.

1906 O'Farrell, Thomas T., F.R.C.S.I., Pathologist, St. Vincent's Hospital; First Assistant in Pathology, University College, Dublin, N.U.I., 44 Waterloo-road. [1]

1883 Olpherts, J. Wybrants, M.D., Medical Officer Downpatrick Dispensary District, The Villas, Downpatrick.

† Ormsby, Sir Lambert Hepenstal, M.D., Ex-President R.C.S., Surgeon Meath Hospital, Surgeon National Children's Hospital, 92 Merrion-square, West, Dublin.

1894 O'Sullivan, A. C., M.D., F.T.C.D., F.R.C.P.I., Lecturer on Pathology Trinity College, 43 Ailesbury-road, Dublin.

† Oulton, Henry W., M.D. Dub., F.R.C.S.I., Chief Surgeon Dublin Metropolitan Police, 41 Stephen's-green, Dublin.

1893 Parsons, Alfred Robert, M.D., Univ. Dub., F.R.C.P., Physician Royal City of Dublin Hospital, 27 Lower Fitzwilliam-street, Dublin. [1]

1895 Peacocke, Geo. J., M.D., F.R.C.P.I., Physician Adelaide Hospital, 2 Fitzwilliam-square.

1903 Peacocke, Reginald C., M.D. Dub., Medical Officer G.P.O., Blackrock District, 4 Avoca-terrace, Blackrock.

1887 Pearson, Charles Yelverton, M.D., F.R.C.S. Eng., Professor of Surgery, Queen's College, 1 Sidney-place, Cork.

1904 Pringle, Seton, M.B., Univ. Dub., F.R.C.S.I., Surgeon Mercer's Hospital, 27 Lower Baggot-street. [1]

1895 Price, Arthur George, M.B., B.Ch. Univ. Dub., Greystones.

† Purefoy, Richard Dancer, F.R.C.S., Ex-Master Rotunda Hospital, 62 Merrion-square, Dublin.

1902 Purser, Frank C., M.D., F.R.C.P., Assistant Physician Richmond, Whitworth, and Hardwicke Hospitals, 20 Lower Baggot-street.

† Redmond, Joseph Michael, M.D., Ex-Pres., R.C.P., Physician to Mater Misericordiæ Hospital, 41 Merrion-square, Dublin.

1905 Rowlette, Robert J., M.D., Physician Jervis-street Hospital, Pathologist Rotunda Hospital, 42 Lower Baggot-street, Dublin. [1]

1892 Rutherford, William, M.D., F.R.C.P.Ed., Visiting Physician District Asylum, Ballinasloe.

1900 Scott, C. Burnett, M.D., 35 Clarinda-park, Kingstown.

† Scott, John Alfred, M.D., F.R.C.S., Professor of Physiology Royal College of Surgeons, 36 Lower Baggot-street, Dublin.

1906 Sheill, J. Spencer, F.R.C.P.I., Ex-Asst. Master Coombe Hospital, 27 Harcourt-street, Dublin. [2]

1886 Smith, Alfred J., M.B., F.R.C.S., Professor of Midwifery, Catholic University, Gynæcologist St. Vincent's Hospital, 30 Merrion-square, Dublin.

1895 Smith, R. Travers, M.D., F.R.C.P.I., Physician Richmond, Whitworth and Hardwicke Hospitals, 20 Lower Fitzwilliam-street. [1]

1901 Smith, Trevor N., F.R.C.S., late Assistant Master Coombe Hospital, 34 Upper Fitzwilliam-street. [1]

† Smith, Walter George, M.D., Ex-President R.C.P., King's Professor of Materia Medica, School of Physic, and Physician to Sir Patrick Dun's Hospital, 25 Merrion-square, Dublin. [1]

1905 Smyth, Joshua Charles, M.R.C.S. Eng., L.R.C.P. Lond., Resident Physician Altadore Sanatorium, Kilpedder, Co. Wicklow.

† Smyly, Sir William J., M.D., Ex-President R.C.P., Ex-Master Rotunda Lying-in Hospital, 58 Merrion-square, S., Dublin.

1908 Solomons, Bethel A., M.B., Univ. Dub., Assis. Master Rotunda Hospital. [1]

1905 Stevenson, Walter C., M.B., B.Ch., B.A.O., Assistant Surgeon Dr. Steevens' Hospital, 21 Upper Fitzwilliam-street. [1]

† Stoker, Sir Thornley, M.D., Ex-President R.C.S., Surgeon Richmond Hospital, Surgeon Swift's Hospital for Lunatics, 8 Ely-place, Dublin.

1902 Stoker, Graves, F.R.C.S., Surgeon Drumcondra Hospital, 46 Rutland-square.

1908 Stokes, Henry, M.D., Univ. Dub., F.R.C.S.I., Clinical Assistant, Meath Hospital, 26 Harcourt-street.

1907 Stoney, R. Atkinson, M.B., B.Ch., Dub., F.R.C.S.I., Surgeon Royal City of Dublin Hospital, 115 Lower Baggot-street, Dublin. [1]

† Story, John Benjamin, M.B., F.R.C.S., Surgeon Royal Victoria Eye and Ear Hospital, Ophthalmic and Aural Surgeon Steevens' Hospital, Professor of Ophthalmic and Aural Surgery R.C.S., 6 Merrion-square, N., Dublin.

1902 Stritch, G. R. Seymour, L.R.C.P. & S. Ed., Governor of Apothecaries' Hall, 13 North Frederick-street.

† Swanzy, Sir Henry Rosborough, M.D., Dub., hon. causâ, Ex-P.R.C.S., Surgeon Royal Victoria Eye and Ear Hospital, Dublin, Ophthalmic Surgeon Adelaide Hospital, 23 Merrion-square, North, Dublin.

1893 Symington, Johnson, M.D. Edin., Professor of Anatomy Queen's University, Belfast.

1895 Taylor, Edward Henry, M.D., Dub., F.R.C.S., Professor of Surgery Dublin University, Surgeon Sir Patrick Dun's Hospital, 77 Merrion-square, Dublin. [1]

1898 Taylor, William, M.B., F.R.C.S., Surgeon Meath Hospital, 47 Fitzwilliam-square.

1897 Thompson, W. H., M.D., F.R.C.S. Eng., King's Professor of Institutes of Medicine, Trinity College, Dublin.

1894 Thompson, Sir W. J., M.D., Univ. Dub., F.R.C.P.I., Registrar-General for Ireland, 59 Fitzwilliam-square.

† Tobin, Richard Francis, F.R.C.S., Surgeon St. Vincent's Hospital, 60 St. Stephen's-green, Dublin.

1893 Tweedy, Ernest Hastings, F.R.C.P., Master Rotunda Hospital. [1]

1900 Tweedy, Herbert, L.R.C.P. & S., Colonial Surgeon.

1901 Watson, Edward J., M.D., F.R.C.P., Anæsthetist and Medical Officer in Charge of the X-ray Department Sir P. Dun's Hospital, Demonstrator in Anatomy T.C.D., 25 Fitzwilliam-place.

1902 Wayland, R. S., F.R.C.S., Physician National Children's Hospital, 67 Harcourt-street.

1901 Westropp, H. G., L.R.C.P. & S., Visiting Surgeon Home of Rest, Merrion, 115 Strand-road, Sandymount.

1904 Wheeler, W. I. de C., M.D., B.Ch., B.A.O. Dub., F.R.C.S., Surgeon Mercer's Hospital, 59 Lower Mount-street. [1]

1900 White, Arthur H., L.R.C.P. & S.I., Professor of Pathology Royal College of Surgeons, Pathologist Meath Hospital and Cork-street Hospital.

1903 Wigham, Joseph T., M.D., Assistant to Professor of Pathology T.C.D., Albany House, Monkstown, Co. Dublin.

1898 Wigoder, Selik, M.D., Leipzig, L.R.C.P. & S. Edin., 5 Harrington-street.

1894 Wilson, T. Henry, F.R.C.P., late Assistant Master Rotunda Lying-in Hospital, Gynæcologist Richmond, Whitworth, and Hardwicke Hospital, 81 Merrion-square.

1898 Winter, W. A., M.D., Dub., F.R.C.P.I., Physician Steevens' Hospital, 17 Fitzwilliam-place. [1]

1893 Woods, Robert H., M.B. Univ. Dub., P.R.C.S., Throat Surgeon Sir P. Dun's Hospital, 39 Merrion-square, Dublin.

1894 Wynne, Albert E., M.D., Univ. Dub., L.R.C.P. & S. Edin., F.R.C.S., late House Surgeon Mercer's Hospital, 27 Westland-row, Dublin.

1884 Yourell, M. J., M.R.C.P., 10 Ailesbury-road, Merrion.

MEMBERS.

1908 Baker, Lily A., M.B., Univ. Dub., 18 Upper Merrion-street, Dublin.

1909 Barrett, J. Boyd, M.B., R.U.I., 6 Upper Fitzwilliam-street.

† Boyce, Jos. W., M.B., Medical Officer Blackrock Dispensary District, St. Kilda, Blackrock, Co. Dublin.

1907 Brontë, R. M., L.R.C.P. & S.I., 28 Harcourt-street.

1903 Charles, Andrew, F.R.C.S.I., 64 Harcourt-street.

1887 Cope, Geo. Patrick, L.R.C.P., L.R.C.S., Medical Officer No. 3 Dispensary District, South Dublin Union, 36 Harcourt-street.

1905 Davys, J. H., L.R.C.P. & S.I., Visiting Physician Children's Hospital, Temple-street, 26 Westland-row.

1892 Day, J. Marshall, M.B., Univ. Dublin, Resident Medical Officer Cork-street Fever Hospital, Dublin.

† Delahoyde, O'Connell J., F.R.C.S., Medical Officer No. 2 District North Dublin Union, 47 Rutland-square, Dublin.

1897 Fleury, Eleonore Lilian, M.D., R.U.I., Richmond District Asylum, Dublin.

1903 Glenny, Edmund, F.R.C.S., Anæsthetist Meath Hospital, 14 Harrington-street.

1900 Goff, A. S., L.R.C.P. & S., Lynton, Dundrum.

1889 Goulding, H. Benson, F.R.C.S., 12 Rathmines-road.

1908 Hargrave, Jennette C., L.R.C.P. & S.I., 8 Upper Mount-street.

1898 Hatch, Richard, L.R.C.P. & S., 146 Pembroke-road.

1897 Hughes, Charles, L.R.C.P. & S., 16 Lower Fitzwilliam-street.

1903 Lynn, Kathleen F., M.B., B.Ch., B.A.O., 9 Belgrave-road, Rathmines.

1905 Pasley, Claude B., F.R.C.S.I., 66 Dalymount, N.C.R.

1908 Pearson, William, M.D., Assistant-Surgeon Adelaide Hospital, 12 Lower Fitzwilliam-street, Dublin.

1898 Redington, John, F.R.C.S., Richmond District Asylum, Dublin.

1885 Shaw, James, L.R.C.S., 93 Talbot-street, Dublin.

1909 Webb, Ella G. A., M.D., 20 Hatch-street, Dublin.

STUDENT ASSOCIATES.

Colquhoun, Miss F., Trinity Hall, Dartry-road.

Drennan, C., Adelaide Hospital.

Hewitt, Miss E., 27 Morehampton-road.

Hunt, H., Adelaide Hospital, Dublin.

Revington, Miss G., 32 Upper Leeson-street

Thomson, T. Mather, 139 St. Stephen's-green.

Watson, T. A., Islington, Terenure.

Woollcombe, Robert Lloyd, LL.D., 14 Waterloo-road, Dublin.

ASSOCIATE.

Dr. G. Halpin, Adelaide Hospital.

RULES.

1. The name shall be, "ROYAL ACADEMY OF MEDICINE IN IRELAND." (1887.)

Constitution.

2. The Academy shall consist of Fellows, Honorary Fellows, Members, and Student Associates.

Management.

3. The affairs shall be managed by a Council, consisting of the President, Ex-Presidents (1893), Ex-General Secretaries (1905), the six Presidents of Sections, the General Secretary and Treasurer, the Secretary for Foreign Correspondence, six Secretaries of Sections, and eight Councillors, being two representatives from the Medical, Surgical, Obstetrical, and Pathological Sectional Councils respectively.

Meetings.

4. The Meetings shall be General and Ordinary.

Publication of "Transactions."

5. The "Transactions" shall be published by the Council, subject to the provisions hereinafter contained.

Original Fellows and Members.

6. All the Members of the present Societies (Medical, Surgical, Obstetrical, and Pathological) shall be Original Fellows or Members, without entrance fee, on payment of the annual subscription on or before 31st December, 1882.[a]

Fellows.

7. Fellows of the Royal College of Physicians of Ireland, and of the Royal College of Surgeons in Ireland, shall be admitted, without ballot, on payment of the entrance fee and the subscription for the current year. All others, being Registered Medical Practitioners not directly or indirectly engaged in the sale of drugs, shall be proposed by two Fellows, and elected by ballot by the Council.

8. Candidates shall be proposed at one Meeting of the Council, and balloted for at the next—one black bean in four to reject.

8A. That all Rules referring to the admission of Fellows, Members, and Student Associates shall be interpreted as referring to Ladies as well as Gentlemen.

Privileges of Fellows.

9. Fellows only shall be eligible for office in the Academy. They shall have the privilege of attending all Meetings of the Academy, of making Communications, and of voting and speaking at such meetings. They shall also receive a copy of the "Transactions."

10. These privileges shall not be exercised by any Fellow in arrear with his subscription.

[a] Those who have paid a Life Subscription to any of the above Societies will be admitted to the privilege of Fellows on payment of Member's subscription.

Honorary Fellows.

11. Honorary Fellows, limited in number to 25, may be nominated by the Council, and elected, on motion at a General Meeting of the Academy by a majority of at least two-thirds of those present and voting.

Members.

12. Any Registered Medical Practitioner may be elected as a Member, the election to be conducted in the same manner as that of Fellows.

Privileges of Members.

13. Members shall have the privilege of attending the Ordinary Meetings of the Academy, of making Communications, and of taking part in debate. They can purchase the "Transactions" at cost price.

Associates.

14. Any Registered Medical Practitioner, temporarily resident in Dublin, may be elected an Associate for the period of one Session. The subscription must be paid before election, and the General Council may elect without notice of motion. Such Associates may attend and speak at the ordinary meetings of the Academy.

Student Associates.

15. Registered Medical Students may be elected Student Associates for the period of one year. The Subscription must be paid before election, and the Council may elect without notice of motion.

Student Associates shall have the privilege of attending the Ordinary Meetings of the Academy.

Annual Subscription.

16. Fellows shall pay £2 2s., and Members £1 1s. Associates and Student Associates shall pay 5s. The Subscription shall become due on the 1st of October in each year, and if the Subscription be not paid on or before the first Meeting in February, the defaulter shall cease to belong to the Academy, unless the delay shall be accounted for to the satisfaction of the Council. No Fellow shall vote at the Annual General Meeting who has not paid his subscription for the year. Registered Medical Practitioners not residing within 15 miles of Dublin are eligible as Fellows of the Academy on payment of the entrance fee, and an annual Subscription of £1 1s. Medical Officers on the active list of the Royal Navy and Army are eligible for election as "Temporary Fellows" of the Academy on payment of an annual Subscription of One Guinea. Such Temporary Fellows shall enjoy all the privileges of Fellows, except that of voting at General Meetings. No Fellow who has not paid his Subscription shall vote for any Candidate for Office at the Annual General Meeting, nor can the name of any Fellow be received by the General Secretary for insertion on the ballot paper as a Candidate for Office unless his Subscription shall have been paid for the current year.

Entrance Fee.

17. After admission of Original Fellows, all Fellows shall pay an entrance fee of £1 1s.

Council.

18. The Council shall meet on the last Friday in the month throughout the Session, or oftener should they see occasion—five to form a quorum.

19. Notice of all Extraordinary Meetings shall be transmitted by the Secretary to every Member of the Council. The President or any five Members of Council may call an Extraordinary Meeting of the Council. The Council shall determine questions by vote, or by division if so demanded, the President having a casting vote only. Any regulation of the Council shall have the force of a law, until submitted to the next General Meeting. The Council shall have the power of filling up any vacancies which may occur in the list of Officers of the Academy, except that of President, before the Annual General Meeting. If a vacancy in the office of President should occur, the General Council shall summon a Special General Meeting of the Academy to fill such vacancy. (1888.)

Sectional Councils.

20. There shall be six Sectional Councils elected by the Annual General Meeting in October, termed respectively—the Medical, the Surgical, the Obstetrical, the Pathological, the State Medicine, and the Anatomical and Physiological Councils.

21. No Fellow shall be eligible as a candidate for election on more than two Sectional Councils, but no Fellow shall be eligible as a candidate for election on both the Medical and Surgical Sectional Councils. (1888.)

22. Each Sectional Council shall consist of the President of the Section and ten Members, one of whom shall act as Secretary to the Section ; except the State Medicine and Anatomical and Physiological Councils, which shall each consist of a President and six Members. (1888.)

Meetings of Sectional Councils.

23. Each Sectional Council shall meet on a fixed day at least one week before the Ordinary Meeting of their Section, three to form a quorum.

Powers.

24. Each Sectional Council shall have the power of making any such arrangements as it thinks necessary to carry on the work of the Ordinary Meetings which are under its charge, provided that such arrangements do not interfere with the general laws of the Academy ; and any Rules laid down by such Council shall have the force of laws at the Ordinary Meetings under its charge until submitted to the General Council.

25. Each Sectional Council shall have the power of filling up any vacancies that may occur among its Members until the Annual General Meeting.

Committee of Reference.

26. The Council shall appoint a Committee of Reference to report upon morbid growths and other specimens exhibited before the Academy ; of this Committee the Exhibitor shall, for the occasion, be a Member.

Officers.

27. A President, to be elected by the Annual General Meeting in October, and to hold office for three years.

28. The Presidents of all Sections shall be elected by the Fellows at the Annual General Meeting, and shall hold office for two years.

29. One General Secretary and Treasurer to be elected at the Annual General Meeting.

30. It is expedient that a fixed salary (of one hundred guineas) shall be paid yearly to the General Secretary in consideration of the fact that the editing of the "Transactions" is part of his duties.

31. One Honorary Secretary for Foreign Correspondence to be elected at the Annual General Meeting. (1888.)

32. The Councillors for each Section to be elected at the Annual General Meeting. Each Sectional Council shall elect two Members to act on the General Council, except in the case of the Sections of State Medicine and Anatomy and Physiology. (1888.)

33. Two Members in each Sectional Council shall retire annually, and be ineligible for re-election for one year, except in the Council of the Section of Anatomy and Physiology, in which only one shall retire. (1896.)

34. Six Secretaries, one for each Section, to be appointed by the Sectional Councils.

35. At all elections after the year 1882, any Fellow desirous of nominating a candidate for election shall, at least ten days before the Annual General Meeting, forward an application to the General Secretary to enter the name of such Fellow on the list of candidates for office, provided that the Fellow so nominated shall have consented to act, and shall have paid his subscription at the time of nomination. Should there be an insufficiency of regularly nominated candidates, the vacancies so created shall be filled up by the Sectional Councils at their first meeting after election. (1891 and 1904.)

36. That all elections shall be by ballot, but Fellows residing more than 15 miles from Dublin, and those incapacitated by illness (to be certified), may record their votes by ballot papers, sent to the presiding officer in sealed envelopes provided for that purpose. A Fellow cannot avail himself of this privilege unless his subscription for the current session has been paid. (1896 and 1904.)

37. That in all elections to the Sectional Councils there shall be affixed to the name of each candidate the number of meetings that he has attended of that particular Section of the Council for which he is now a candidate. (1898.)

Duties of Officers.

38. *The President* shall preside at the Annual and Special General Meetings and at General Council Meetings. In the absence of the President, the Chairman shall be appointed by the meeting. (1888.)

39. *The Presidents of Sections* shall preside at the Ordinary Meetings of the Academy, and shall also preside at the Sectional Council Meetings. In the absence of the President, the Chairman shall be appointed by the meeting. (1888.)

40. *The General Secretary* shall attend all General Meetings of the Academy and General Council. He shall take minutes of such meetings, to be read at the following meeting.

41. He shall receive and have charge of all papers intended for publication in the "Transactions" of the Academy, after they have been handed over to him by the Secretaries of the several Sections.

42. He shall, on receiving notice from the Secretary of a Section, send out to all the Members notices of the title or titles of the paper or papers for the next Ordinary Meeting, with the name or names of the authors, and, so far as possible, of the subjects for Exhibition, with the names of the Exhibitors.

43. He shall arrange for the Exhibition of specimens and the reading of papers, which are to be forwarded to the Academy by those who are absent, or are not members.

44. The General Secretary and Treasurer shall receive all moneys, and lodge the same in bank to the account of the Academy, and all cheques shall be signed by the Treasurer and one other Councillor.

45. The Accounts shall be audited by two Fellows, not Members of Council, to be appointed by the President at some meeting previous to the Annual Meeting.

Duties of Secretaries of Sections.

46. To attend the Meetings of the Council of the Section and the Ordinary Meetings of the Academy, under the management of said Council, and to take minutes at such meetings, to be read at the next following meeting of that Section.

47. To keep such papers as the Sectional Councils recommend for publication, for the purpose of handing them over to the General Secretary.

48. To inform the Secretary of the Committee of Reference of any specimens referred to that Committee, and to transfer the specimens to that Secretary.

49. To give notice to the General Secretary, one week previously to the meeting, of the titles of papers for the evening, the names of the authors, and, so far as possible, the objects for Exhibition, with the names of Exhibitors, so that the General Secretary may inform the Members.

Meetings.

50. The Annual General Meeting to take place on the second Friday in October, for the election of Officers and Members of Council, and for the general business of the Academy.

51. Due notice of the meeting shall be given by the Secretary to all Members at least three weeks previously. (1891.)

52. No motion involving a change of these Rules shall be brought before this meeting except one week's notice thereof shall have been given by the Secretary to each Member.

53. The President may—and shall forthwith, on receiving a requisition signed by seven Fellows, at any time—on giving one week's notice, summon a Special General Meeting, for the consideration of particular business, the nature of which must be specified in the letter of summons convening the meeting, and at such meeting no other business can be transacted. In the event of the President being unable, from any cause, or declining, to summon a Special General Meeting of the Academy, it shall be in the power of the General Council to summon such meeting. (1888.)

Ordinary Meetings.

54. The communications to be submitted to the Ordinary Meetings shall be grouped under the following heads :—Medicine, Surgery, Pathology, Obstetrics, State Medicine, and Anatomy and Physiology ; and the conduct of such meetings shall be in the hands of the several Sectional Councils, each Sectional Council to have the management of the Ordinary Meeting in rotation, as arranged by the General Council. (1888.)

55. The Ordinary Meetings shall be held on every Friday evening, from the last Friday in October until the last Friday in May, inclusive, at eight o'clock, except during the Christmas and Easter recesses.

56. All Fellows, Members, and Student Associates attending the meetings shall write their names in the attendance book.

57. Any Fellow or Member may introduce two Visitors by cards obtained from the Sectional Secretaries.

58. Officers of the Army or Navy Medical Departments shall, on presenting their cards, be admitted to the Ordinary Meetings of the Academy.

59. No communication shall exceed twenty minutes in its delivery, nor any speech thereon ten minutes, except by permission of the Chairman. No one shall speak twice upon the same communication, except the author, who has the right of reply.

60. A paper by any other than a Fellow or Member of the Academy shall not be read before the Academy unless the author of such a communication shall have obtained permission to do so from the Council of the Section before which the communication is proposed to be read. (1892.)

Ordinary Meetings.—Order of Business.

61. (1.) Chair to be taken at 8 30 p.m.

(2.) Chairman to read list of specimens, &c., exhibited by card, together with the names of the Exhibitors.

(3.) No Pathological Specimen shall be exhibited at any Section other than the Pathological and Obstetrical, except by card. This Exhibition shall not exclude any subsequent communication regarding it at the Pathological Section.

(4.) There shall be no Exhibition of Specimens by card in the Pathological Section.

(5.) Any member shall have liberty to exhibit any recent specimen at any of the meetings of the Obstetrical Section, provided it illustrates any question in gynæcology.

(6.) At the meetings of the Obstetrical Section recent specimens may be exhibited, and the President may invite discussion thereon, provided that such exhibition of specimens or discussion, if any, thereon, must terminate at 9 o'clock, p.m., but that, if necessary, they may be resumed after the papers for the evening have been read and discussed.

(7.) Chairman to ask if any member has any observations to make or motion to propose relative to any living specimen on the List of Exhibition.

(8.) Chairman to call upon the author of the first paper on the list to read his paper.

(9.) Chairman to call upon members to discuss the paper, or, at his discretion, to take any other paper or papers on the list relating to the subject, and have the discussion subsequently on all such papers collectively.

(10.) When the last paper has been discussed, the Chairman to ask if any member desires to speak upon any of the specimens exhibited by card.

(11.) After the discussion upon any specimen, the Exhibitor has the right of reply.

Regulations regarding the Exhibition of Specimens by Card.

62. (1.) Any member may exhibit by card at any Ordinary Meeting, except at the meeting of the Pathological Section. At the meetings of the Pathological Section all specimens must be presented and described *vivâ voce*, and debate may be invited thereon.

(2.) Notice shall, if possible, be given to the General Secretary, or the Secretary of the Section, on or before the previous Ordinary Meeting.

(3.) Specimens must be in the room at 7 45 on the night of Exhibition.

(4.) Specimens for Exhibition by card shall be open for inspection at 8 p.m.

(5.) A card, containing all particulars for publication, shall be placed with the Specimen. Cards for this purpose are to be obtained from the Secretary.

(6.) The Exhibitor should be present, and he shall furnish further details if asked for.

(7.) Every Exhibitor shall submit the Specimen or Specimens on view to the Committee of Reference, if the meeting so decide.

Exhibition of Pathological Specimens.

63. No lengthened reference to treatment shall be allowed upon any Specimen, except by the express permission of the Chairman. Whenever it has been agreed that a Specimen exhibited at a Sectional Meeting of the Royal Academy of Medicine in Ireland shall be sent to the Reference Committee to report thereon as to its nature, the Exhibitor is to retain the custody of the specimen until he shall be summoned to a meeting of said Committee to be convened by its Secretary, on an early day, when he will attend and submit it for examination. (1889.)

By-laws concerning "Transactions."

64. The "Transactions" shall consist of such Communications made to the Academy by or through Fellows or Members as may be deemed by the General Council suitable for publication; also, of discussions of importance or interest arising out of such Communications.

65. All Communications accepted by the Academy become the property of the Academy, but authors may also print their Communications, subsequent to the reading of the same before the Academy, in any publication in addition to the "Transactions." Papers shall be handed to the Secretary of the Section immediately after they have been read. (1891.)

66. The "Transactions" for the year shall be presented to all Fellows of the Academy who have paid their Annual Subscriptions.

67. The "Transactions" may be purchased by Members at cost price.

68. The Publication Committee of each Section shall meet not later than the Tuesday after each meeting of the Section, for the purpose of abstracting the proceedings—the abstract to be placed in the printer's hands on same evening, and forwarded to the editors of medical journals with the least possible delay. (1888.)

69. Contributors of papers may send their papers to the Academy printer early enough to allow of their being put in type before the meeting, provided the author be responsible for the cost of same should the General Council deem the communication not suitable for publication in the "Transactions."

70. That on the evening of the day of meeting of the Sectional Council when the papers for the next meeting have been decided upon, a circular be sent to each contributor by the Secretary of the Section informing him :—

(1.) That he is expected to be ready or else take his place at the bottom of the list.

(2.) That he must have an abstract ready with his paper, otherwise he will be noted in the published proceedings in such form as the Publication Committee think fit.

71. The General Council is empowered to defray the expenses in whole or in part of any illustrations which it may consider advantageous to the elucidation of the papers published by the Academy.

72. An abstract (prepared by the author) of each communication made at the Academy, along with a report of the discussions thereon, shall be furnished to the editors of such medical journals as may desire to publish them, and the authors of such communications shall be empowered to publish their papers *in extenso* in any periodical or periodicals they may think fit, such communications also to appear in the "Transactions," provided the Council consider them worthy of insertion.

Expulsion of Fellow or Member.

73. Expulsion of a Fellow or Member can take place only at a General Meeting of the Academy, on the motion of the Council, if two-thirds of the Members present shall vote for the same by ballot. Of such ballot the Council must give at least fourteen days' notice in writing to every Fellow of the Academy.

New Laws.

74. New Laws, or alterations in existing Laws, can be proposed only at the Annual General Meeting. Any Fellow proposing such alteration shall give notice to the General Secretary at least ten days before the General Meeting in October.

REPORT.

The General Council reports that for the Session 1908–9 the number of Fellows was 184, of Members 23, and Student Associate 1. The Fellows and Members numbered exactly the same as for the previous Session.

In the matter of finances the credit balance to the Academy in the National Bank stands at £225 2s. 3d., as compared with £174 15s. 4d. of the previous Session.

The attendances at the Sectional Meetings during the past Session amounted to an aggregate of 731, as against 753 for the Session 1907–8.

The Tuberculosis Prevention (Ireland) Act, 1908, was placed on the Statute Book during the year, and your Council has to report with regret that Mr. Birrell, Chief Secretary for Ireland, refused to amend the Bill on the lines adopted by the Academy of Medicine at the Special General Meeting held on July 9th, 1908.

During the year the President of the Academy, in conjunction with the Presidents of the Royal College of Physicians and the Royal College of Surgeons, took an active part in bringing to a successful issue the formation of a Dublin Branch of the Medical Research Defence Society.

Your Council had under consideration the General Anæsthetics Bill, 1908, and passed a resolution in favour of leaving the administration of Anæsthetics in the hands of Registered Medical Practitioners exclusively, as was provided in the Bill.

The Council records with deep regret the loss the Academy has sustained by the death of Professor Alexander Fraser and Dr. Michael Strahan. Professor Fraser had occupied the Chair of Anatomy in the Royal College of Surgeons for a period of twenty-five years, and during that time he had done much original work. He became a Fellow of the Academy in 1887, was a Past-President of the Section of Anatomy and Physiology, and to this Section he communicated many of his most important papers. The Council also reports with regret the death of Professor Cunningham, of Edinburgh, who was a former distinguished Fellow of the Academy, and had occupied the position of President of the Section of Anatomy and Physiology.

24*th September*, 1909.

General Treasurer's Report for the Session 1908–1909.

RECEIPTS.

	£	s.	d.
To Balance in Bank - - -	174	15	4
,, Subscriptions and Entrance Fees -	385	12	0
,, Dividends on £596 2s. 4d. 2½ per cent. Consolidated Stock and £155 1s. 10d. New 2½ per cent. Government Stock	18	0	8
,, Sale of Transactions - - -	3	0	0
Total - - - -	£581	8	0

EXPENDITURE.

	£	s.	d.
By General Secretary - - -	105	0	0
,, Printing, Stationery, and Postage -	58	6	6
,, Transactions, Vol. XXVII. - -	97	2	9
,, Royal College of Physicians - -	15	15	0
,, Royal College of Surgeons - - -	15	15	0
,, Reporters - - - - -	28	7	0
,, Servants - - - - -	20	10	0
,, Groceries and Milk - - -	11	7	6
,, Care of Microscopes - - -	2	0	0
,, Sundries - - - - -	2	2	0
,, Balance in Bank - - - -	225	2	3
Total - -	£581	8	0

The Capital is invested in the names of Sir WILLIAM THOMSON, Dr. WALTER G. SMITH, and DR. JAMES CRAIG.

We have examined the Accounts and Vouchers, and certify the same to be correct.

24th September, 1909.

CHARLES M. BENSON, M.D.
HENRY STOKES, F.R.C.S.

Volume XXVII. of the "Transactions" has been forwarded to the following:—

IRELAND:—

Queen's University	Belfast.
Queen's College	Cork.
Do.	Galway.
Medical Press	Dublin.
National Library	Do.
Royal College of Physicians	Do.
Royal College of Surgeons	Do.
Royal Irish Academy	Do.
Royal Dublin Society	Do.
Royal University	Do.
Trinity College	Do.

ENGLAND:—

Birmingham Medical Review	Birmingham.
Medical Institute	Do.
Bristol Medico-Chirurgical Journal	Bristol.
The University Library	Cambridge
University of Durham College of Medicine	Newcastle-on-Tyne.
Liverpool Medico-Chirurgical Journal	Liverpool.
Journal of Obstetrics and Gynæcology, 60 Chandos-street, W.C.	London
King's College	Do
Lancet	Do.
Library, British Medical Journal, 429 Strand	Do.
Lister Institute of Preventive Medicine, Chelsea Gardens	Do.
Medical Magazine, 44 Bedford-row, W.C.	Do.
Medical Review, 66 Finsbury Pavement, E.C.	Do.
Official Year Book of Scientific and Learned Societies, Exeter-street, Strand	Do.
Public Health, 1 Upper Montague-street, Russell-sq., W.C.	Do.
Royal College of Physicians	Do.
Royal College of Surgeons	Do.
Royal Society of Medicine, 20 Hanover-square	Do.
University of London, South Kensington	Do.
University College, Gower-street, W.C.	Do.
The Manchester Medical Society, The Owen's College	Manchester
Victoria University	Do.
The Bodleian Library	Oxford

SCOTLAND :—

University	Aberdeen.
University College	Dundee.
Royal College of Physicians	Edinburgh
Royal College of Surgeons	Do.
The Advocates' Library	Do.
University	Do.
Faculty of Physicians and Surgeons	Glasgow.
University	Do.
Do.	St. Andrews.

EUROPE :—

University	Amsterdam.
Naturforschende Gesellschaft	Basel.
Kgl. University Library, Dor theen Strasse	Berlin.
University College	Bologna.
Académie Royale de Médecine de Belgique	Brussels.
L'Année Chirurgicale	Do.
Société Belge de Chirurgie	Do.
Université Libre	Do.
University College	Christiania.
Académie de Médecine	Copenhagen.
La Grece Medicale, Syra	Greece.
Real Institute Bacteriologico Camera Pestana	Lisbon.
University College	Madrid.
Archivio di Ortopedia	Milan.
Académie de Médecine	Paris.
Archives Orientales	Do.
Revue de Chirurgie	Do.
University of Paris	Do.
Imperial University	St. Petersburg.
Royal Caroinska Med. Chir. Institution	Stockholm.
University Library	Strasburg.
Archivio Italiano di Otologia	Turin.
Medical Society, Royal University	Upsala.
University	Vienna.
Naturforschende Gesellschaft	Zurich.

AMERICA :—

Johns Hopkins University	Baltimore.
Journal of Medical Research	Boston, Mass.
Brooklyn Medical Journal	Brooklyn.
McGill University	Montreal.
Academy of Medicine, 17 West 43rd Street	New York.
University	Do.

AMERICA—*con.*

American Medical Association	Chicago.
College of Physicians	Philadelphia.
University of Pennsylvania	Do.
University Laval	Quebec.
University of Toronto	Toronto.
Charlotte Medical Journal, Charlotte, N. C.	United States.
Library, Surg.-General's Office, Washington	Do.
Medical Library Association, 19th and Stout-streets, Denver, Colorado	Do.

AUSTRALASIA :—

University	Adelaide.
Do.	Auckland.
Do.	Melbourne.
Inter-Colonial Medical Journal	Do.
University of Otago	New Zealand.
The Australasian Medical Gazette	Sydney, N.S.W.
University of Sydney, care of Young J. Pentland, 38 West Smithfield, London, E.C.	Do.

ASIA :—

Bombay University	Bombay.
University	Calcutta.
Imperial University Library	Tokio, Japan.
Library, Bureau of Science	Manilla, P.I.

TRANSACTIONS

OF THE

ROYAL ACADEMY OF MEDICINE IN IRELAND.

SECTION OF MEDICINE.

ON SOME RELATIONS OF PHYSIC TO PHYSICS, AND THEIR BEARING UPON THERAPEUTICS.

By WALTER G. SMITH, M.D.;
Ex-President of the Royal College of Physicians of Ireland.
Physician to Sir Patrick Dun's Hospital.

[Read in the Section of Medicine, October 29, 1909.]

In the desultory remarks which I offer to you on this occasion I will assume that the term physics includes chemistry. For it is evident that physics, no longer content with the molecule, has now annexed the sacred atom of the chemist.

The atomic theory has scarcely passed its centenary, and yet we have already become familiar with phenomena which prove that the immutable and indivisible atom of the 19th century has lost these attributes. Chemistry, while its ordinary practical course is untouched by the new revelations, has come to recognise that the atom is a microcosm in structure, and conceals an amazing storehouse of energy, electrical in its nature.

In the early dawn of medicine, theology and therapeutics—the former spelt with a big T—were closely interwoven. By

degrees the ill-assorted alliance has been severed, and, in later days, all earnest cultivators of medical science have done their best to emancipate it from the thrall of superstition and of mere dogmatic authority.

This is the true Renaissance of Medicine, and the spirit of free inquiry, or moderate doubt, is the distinctive feature of modern thought. We can the more truly reverence the great Fathers of Medicine, Hippocrates and Galen, although we do not bow down and worship them as was done for many centuries. In our own times we witness the rash endeavours of certain clerics, in America and elsewhere, to re-engage in practical therapeutics, and, with the self-assurance born of ignorance and conceit, they intrude where they ought not, and meddle where they can do nought but mischief. As Osler ("The Treatment of Disease," *Brit. Med. Journ.*, July 24, 1909) aptly quotes from Fuller:—"Circe and Æsculapius were brother and sister, and the wiles of the one are very apt to entrap the wisdom of the other."

Homœopathy had for a time a sickly existence, and has nearly perished of inanition. But its vacant throne is, in part, occupied by that audacious fraud and moral influenza, yclept Christian Science, a pursuit the "christianity" of which is as bizarre as its "science" is contemptible. A woman lately died at Worthing who had been suffering from consumption for five years. At the inquest it was stated that she had been in the hands of the Christian Scientists, who charged her a reduced fee of five shillings weekly for praying twice daily. The regular fee was £1 1s. (*Irish Times*, Sept. 14, 1909.) To Christian Science may be fitly applied a "bull" once perpetrated in the House of Commons—"It is a lie, sir, and it is high time we nailed this lie to the mast."

It might be supposed that, with the advance and spread of scientific knowledge, quackery and superstition would die

out. Yet the experience of the ages teaches that man has an ineradicable hankering after the unknown and the unknowable, and is ever ready to pin his blind faith to the incredible and the marvellous.

Man is, by nature, an idolater, and he revels in that greatest of all human capacities—the capacity for self-deception (Osler).

While we gladly allow that chemistry has made magnificent contributions to our stock of remedies, the very facility with which organic syntheses can be effected has its disadvantages.

Of the innumerable new remedies which are daily showered upon us, most of them are like falling stars which flash across our path for a moment, and leave behind not even a speck of therapeutic dust. A few, like meteorites, cling to earth and become part of our possessions.

We are ready also to admit our obligations to the pharmaceutical skill of the great drug firms, but they now presume, under an ill-disguised humility, to teach us our business, and, by their commercial wits, they batten upon the witlessness of the public—lay and medical.

They invade us in our consulting hours at home, and bombard us with their pseudo-scientific missiles—heralded by a flourish of fearful chemical formulæ, and so degrade the progress of real therapeutics to a vulgar commercial competition.

"Where is boasting, then? It is excluded."

Let us turn now to other thoughts.

An analogy may be traced between the parallel courses of modern physiology and modern chemistry.

For a time the investigations of physiologists ran largely on systems and tissues: those of chemists on systems, groups, and types. During recent years the tendency in each is to return to the closer study of their respective units—that is to say, the cell for physiology, the atom for chemistry and physics. In each case the unit turns out to be a highly complex

structure, with infinite potentialities, and the atomic hypothesis plays the leading part in the science of to-day.

The two great laws which serve as beacon lights in the investigation of natural phenomena are the generalisations known as the laws of thermodynamics—

(*a*) The principle of conservation of energy—the first law.

(*b*) The doctrine of the transformations, or degradation of energy—the second law of thermodynamics.

These two principles are obeyed throughout the whole range of organised nature, and, as Benjamin Moore shows so clearly in his interesting and suggestive articles in "Recent Advances in Physiology and Biochemistry," the living cell is an energy machine or transformer.

To comprehend and follow these exchanges of energy is largely, for biologists, the task of the new branch of study termed physical chemistry, which may be regarded as the product of the cross-fertilisation of physics and chemistry.

So far as we can ever hope to penetrate the mystery of life, it is surely only by the aid of such inquiries and experiments. This was dimly perceived and felt by the older seekers after truth, as is seen in the gropings of iatro-mechanics and iatro-chemistry in the 17th and 18th centuries.

Methods of physico-chemical therapeutics—*e.g.*, massage and hydrotherapeutics—reach back to antiquity, and one of the earliest recorded examples of hydrotherapeutics is related in the story of the cleansing of Naaman's "leprosy."

In my address to this Section last year upon Baths and Mineral Waters, I attempted a brief and inadequate sketch of the physico-chemical principles which lie at the foundation of scientific balneology. If we were to take seriously the claims of the proprietors of the different mineral waters, they would have us to believe that each spring is a veritable pool of Bethesda. But the pool needs to be stirred with the rod of science.

In Starling's recent book on "The Fluids of the Body," we have an exposition by a master-hand of the physico-chemical factors which appear to govern the intake, the output, the distribution, and the exchanges of the body liquids. Chemical changes are at the root of all life, and if we seek for a criterion of animate substance we find that metabolism is *the* distinction between living and lifeless substance.

The possession of the highly complex protein molecule is a definite mark of distinction of the living organism as contrasted with all inorganic bodies (Verworn, *Allgem. Physiologie*, 1909).

Chemical energy is the source of all other forms of energy in the organisation. By its transformations are derived mechanical energy, light, heat, and electricity.

In chemistry, as pointed out by H. E. Armstrong, everything tends to show that function and structure are most closely connected—odour, taste, colour, physiological effect, are specific rather than general properties, each conditioned in its special variety by some special structure (Presidential Address to Section B., Chemistry, at the British Association Meeting in Winnipeg, 1909).

The influences of changes of temperature upon the phenomena of life are obviously of high significance, and they have a practical interest for physicians in connection with the pathology of pyrexial affections.

We have learned that the range of temperature compatible with preservation of life is far greater than was formerly thought. The very surprising fact has been established that the living substance of the cells of animals so high in the scale as fishes and frogs can be frozen into ice without losing its capacity for restoration to vital activity.

As for bacteria, Macfadyen has shown that a temperature of 252° C.—*i.e.*, only 21° above absolute zero—failed to destroy

the life of certain bacteria even after ten hours' duration of the experiment. Conversely, he has also shown that there are "thermophilous" bacteria, whose *optimum* lies between 50° to 65° C.; and some cultures will grow at a temperature of 72° C. (161.6° F.).

The equilibrium point of a metabolic process can be shifted by alterations of temperature. Take, for example, the potato.

The conversion of starch into sugar is an exo-thermic reaction—*i.e.*, is attended with extrication of heat. Therefore, in accordance with van t'Hoff's principle of mobile equilibrium, a rise in temperature will favour the reverse, or endo-thermic process—*i.e.*, the regeneration of starch. Below 5° C. sugar is formed in a potato from starch: if the temperature rises, starch will be re-formed from sugar. Similarly, evergreen leaves are, in winter, often free from starch, but contain grape sugar. If brought into a warm room, the sugar vanishes and starch takes its place (Höber, *Physikal. Chem. der Zelle und der Gewebe*, 1902).

So we see that cell physiology is the very root and basis of all physiology, just as atoms are the foundation stones of chemistry.

Calorimetry, coupled with metabolic investigations, leads to the deduction that the energy expended by the body can be *quantitatively* accounted for by the stores of potential energy contained in the food.

Where, then, is there room for that mysterious *vital force*, or *vital energy*, or *biotic energy* (B. Moore), which, like Melchisedek, has neither father nor mother, and which has so long dominated and enfeebled physiological thought. How do we gain by using a hazy conception expressed in an indefinable word?

The doctrine of the "vital force" is really only a mythological way of expressing the amazement which the unique

character of organic phenomena excited (Höffding, *Outlines of Psychology*, 1908).

When we say that physiology is and never can be aught else than the physics and chemistry of the living organism, this is not to assert that the forms of energy in living matter are *identical* with the forms of energy exhibited in non-living structures. And, undoubtedly, there are important phenomena, especially in connection with the nervous system, which lie outside the sphere of experimental inquiry, and belong to pure psychology.

The mystery of life is, probably, an insoluble problem in its ultimate reduction, and, for my own part, I am quite willing to accept Du Bois Reymond's celebrated dictum—*Ignoramus et ignorabimus.*

The fluid nature of living substance is its most important physical property. Hence, the deep significance of the study of colloids, of osmotic phenomena, ionic dissociation, and the varying and complex functions of membranes and of surfaces of separation between the cells of the plant or animal.

From a purely physical point of view living substances may almost be regarded as *animated water* (H. E. Armstrong). Or, we may say with Loeb, that all life phenomena are ultimately due to motions or changes occurring in colloidal substances, for nature works with simple tools.

Another illustration of physico-chemical processes is afforded by catalytic, or ferment, phenomena, which play a dominant part in our physiology, and it is no exaggeration to say that the wheels of life are lubricated by the oil of catalysis. In Dr. Bayliss's recent monograph on "The Nature of Enzyme Action" we have an excellent and lucid exposition of the fundamental problems connected with enzyme action.

In the highest sphere of inquiry—viz., that of mental processes—medical men are not much attracted by the barren speculations of scholastic psychology. Long ago that monu-

mental genius, Johannes Müller, tersely expressed this feeling in the phrase—*Psychologus nemo nisi physiologus.*

The strange and weird phenomena of mental disease, of hypnotism, suggestion, heredity, instinct, and so forth, will, I am convinced, never be elucidated except by the labours of physiological psychologists, who do not shut themselves up in their studies to dream dreams, but avail themselves, as opportunity offers, of the aids which *comparative* anatomy and physiology, coupled with experimental science, alone can afford. In this connection I may refer to the excellent text-book of "Experimental Psychology" by Mr. Charles S. Myers (1909).

Human happiness and well-being have been and will be further advanced by physiologists, and for the furtherance of this laudable end it has been proved up to the hilt that it is absolutely necessary to utilise experiments on animals, humanely conducted, a method too often stigmatised by the injurious and inapt term, vivisection.

Sir Samuel Wilks tells a story of an old lady he heard declaiming against the horrors of vivisection. She went on to say that there was no cruelty or dreadful thing that scientific men would not do to find out how things acted. There was nothing that God had created but what they would destroy to look at its inside; but, "thank God," she ended, "they can't touch the beautiful aurora borealis!" (*Brit. Med. Journ.*, July 31, 1909.)

Already a beginning has been made to create a mechanics of the brain and higher nerve centres, and its future is rich in promise.

For example, let us take the law of mass action, which postulates that the course of a chemical reaction depends not only upon the mode of the chemical affinities, but also on the relative quantities of the interacting bodies. Suppose we stimulate a muscle so that its metabolism is continuously increased—*i.e.*, the dissimilation stage of its metabolism is

Gamut of Ether Waves.

W. lengths in microns, μ

0.1 μ } Ultra-violet.
0.2 }

0.4 } 1 octave of light waves.
0.8 }

1.6 } 7 octaves of dark heat.
3.2 }
6.4 }
12.8 }
25.6 }
51.2 }
102.4 }

204.8 } 5 octaves of unknown radiation.
409.6 }
819.2 }
1638.4 }
3276.8 }

6553.6 4 mm. = very short Hertz wave.

13 mm. } Hertz radiations.
26 }
52 }
104 }
208 }
416 }
832 }
1664 }
3328 }
6656 }
13312 }

Longer waves up to 1,000 feet or more, in wireless telegraphy. Hertz waves probably include 16-17 octaves.

excited—then, automatically occurs a new formation of living substance—*i.e.*, a secondary excitation of the assimilation phase of its metabolism is produced. Thus the deficit is equilibrated, the metabolic equilibrium is restored, the biotonus once more = 1. The converse will, of course, happen if the assimilation process is augmented by an increased supply of nutriment.

Thanks to this principle we are able to repair the inroads of fatigue, to overcome disease, and to prolong life (Verworn, *loc. cit.*, p. 595).

Once more, the principle of interference of stimuli is one of far-reaching importance in nerve-physiology and nerve-therapeutics, and the phenomena of inhibition can, perhaps, be best interpreted in terms of interference. A familiar example of inhibitory influence is afforded by the facts of snake-charmers, as practised in Egypt and the East from the time of Moses and Aaron until now.

Again, what a vast and fruitful field of inquiry and of practical therapeutics is opened up by investigations upon the effects of auto-intoxications, intestinal and otherwise, upon mental functions.

Have we not gained light and won therapeutic triumphs by a study of the effects on the nervous system of alterations in the internal secretory glands, of the thyroid and parathyroid, of the adrenals, and of the sexual glands.

But inexorable time presses, and I must not tax your patience with more than one other thought.

By far the most striking illustration of the influence of external conditions upon vital processes is afforded by the different forms of radiant energy, considered as stimuli.

Until recently we were acquainted only with the action of luminous and thermal rays as physiological stimuli, and we have reviewed one or two points in connection with heat rays.

Time forbids to enter upon the fascinating topic of the

potent influence of light-rays, considered apart from heat-rays, and it is unnecessary to dwell upon the services rendered by the Finsen light.

Speaking generally, it may be stated that the physiological activity of light-rays is inversely proportional to their wave-length. Hertel has lately made special and suggestive investigations upon this subject. He arrives at the conclusion that in all probability the general action of light-rays upon living substance depends, in the first instance, upon modifications in the oxygen-exchange (Table of Wave Lengths).

Fourteen years ago (1895) a new world was disclosed to our ken by the discovery of Röntgen rays, and this was quickly, and logically, followed by the revelation of Becquerel rays, and of the radio-active bodies—viz., Uranium, Radium, Thorium, &c.

Experimental facts have forced us to widen our conception of a "ray." We have now to recognise two kinds of rays—

(1) Streams of particles, of different mass, and with velocities ranging from 15,000 miles per second up to nearly that of the speed of light—*e.g.*, α and β rays from radium.

(2) Electro-magnetic vibrations of the ether.

No conclusions of any weight can yet be drawn as to the *general* physiological action of Röntgen rays, and I pass over the now familiar applications of them to diagnosis and treatment in medicine and surgery.

Among radio-active bodies, especially radium, the α-rays appear to be the most potent, both chemically and physiologically. From a therapeutical standpoint they can scarcely yet be taken into account, for they are held back by slight obstacles, even by the thinnest mica, glass, or aluminium covering of the tube or vessel which contains the radium salt. The α particles are completely stopped by a sheet of note-paper, while the γ-rays from radium can be easily detected after traversing twenty centimeters of lead (Rutherford).

It is, perhaps, possible that we may learn to harness α-rays to our service by employing either polonium, which yields only α-rays, or we may perhaps utilise the positive so-called "canal rays" of a Röntgen tube.

A position of unquestionable utility has been gained by the employment of the more penetrating β and γ-rays, which hang together, and the prospects of radio-therapy are undoubtedly bright. The relative penetrative powers of the α, β, and γ-rays may be approximately represented by 1 : 100 : 10,000.

It is probably true that radium can effect a cure in certain cases where X-rays fail, but it is premature to generalise.

Everyone here has, doubtless, read some of the already huge literature of the subject, and will recall Sir F. Treves' lecture on "Radium" (*Brit. Med. Journ.*, Feb. 6, 1909), and the excellent address lately delivered by Dr. Wickham, of Paris, before the British Medical Association in Belfast.

Wickham's pioneer researches were carried out in the Radium Institute in Paris, of which he is Director. Thanks to the enlightened foresight of the King, and the generous aid of Lord Iveagh and Sir E. Cassel, we shall shortly have a Radium Institute at home, which will, we feel assured, make weighty contributions to the new science, and add invaluable weapons to our therapeutical arsenal.

A CASE OF SPLENO-MEDULLARY LEUKÆMIA TREATED BY X-RAYS.

By MAURICE R. J. HAYES, F.R.C.S.I.;

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[Read in the Section of Medicine, November 26, 1909.]

Case.—M. A. M'E., aged forty-two; occupation, housewife: married; no children.

History of Present Disease.—In December, 1908, she first noticed a hard swelling in the left side of her abdomen below her ribs, and her sides and back became sore and tender; the soreness was so great that she could not bear the pressure of her clothes. Early in February, 1909, her abdomen got so large that her clothes did not fit. At this time her colour assumed a lemon-yellow tint, and she was weak and breathless on exertion. She now also suffered a great deal from headaches, dizziness, and constipation, and her vision was cloudy. She rapidly lost flesh, and her feet used to swell, especially at night. She also noticed blotches, bluish in colour, on her hips and hands; these were very tender to the touch, and they came and went irregularly, remaining for varying intervals; at times she was quite free from them. She never had any hæmorrhage.

Previous History.—She was always healthy, but anæmic. In 1901 she was treated by Dr. Robert Farnan, in the Mater Misericordiæ Hospital, for a retroverted uterus (curetting, replacement, and pessary).

In August, 1906, she suffered from an attack of what was called acute rheumatism, when she was confined to bed for three months, but her joints were never painful.

In January, 1907, she was re-admitted to the Mater Hospital

complaining of pain in her back and down her legs. She had some pains in her arms, and while in hospital she developed a purpuric rash. She had a left femoral hernia, and was very anæmic. The pessary, which was inserted in 1901, had been dispensed with for some years, and the uterine displacement recurred, for which she was again treated as before. No operative treatment was ever deemed advisable to rectify this condition on account of the very anæmic state of the patient. The pains in her legs did not respond to anti-rheumatic remedies, but they greatly improved under the administration of arsenic. Sometimes her temperature was slightly elevated at night—99.5° or 100°—at other times it was quite normal. Urine normal. There was some frequency of micturition, but this was removed by replacement of the uterus. Since her girlhood she had always suffered from dysmenorrhœa and anæmia—the former was relieved by the replacement of her uterus in 1901. Menstruation was regular until December, 1908. She was unwell once in February and once in May, 1909. She has menstruated regularly since May, except that she missed one period in October.

Family History.—Mother alive and healthy. Father died of cancer of the stomach, aged fifty. She has one brother and one sister alive and healthy. Two brothers and two sisters died in infancy from whooping-cough and measles.

Condition in February, 1909.—Anæmia well marked; there is a yellowish tinge in her skin, and she is grey and wasted. She suffers very much from breathlessness and palpitation; there is swelling about her legs, and she has hæmic murmurs. Her spleen is greatly enlarged, and it extends on the right side for two inches beyond the middle line, and its lower limit to about three inches below the umbilicus and to the crest of the left ilium. It is very hard in consistence, and its lower and anterior margins can be distinctly felt through the abdominal wall. Liver but very slightly enlarged, and there are no enlarged lymph glands. The blood count on February 15, 1909, was as given below (see Table I.).

Treatment.—At Dr. Farnan's request I began X-ray treatment on February 20, 1909, and I may here remark that from that time till now she has had no medicinal treatment whatever for leukæmia, nor is she taking any drug at present.

For purposes of treatment I divided the surface of the abdomen over the enlarged spleen into four circular areas, each having a diameter of ten centimetres. These areas I shall name as follow :—" The lower anterior," " the upper anterior," " the left epigastric," and " the mid-axillary," over the ninth, tenth and eleventh ribs in the mid-axillary line. Each of these areas, beginning at the lower anterior, was irradiated consecutively in the order in which they are given, the cycle beginning at " the lower anterior." A hard tube enclosed in a shield opaque to the rays, with a diaphragm of 10 centimetres in diameter, having an alternative spark-gap of 4 to 5 inches, was used, with .5 milliampère going through the tube ; the average duration of the exposures given was 18½ minutes (*i.e.*, 1 Sabourand = 4 to 5 H of Holzknecht's scale) ; the distance of the anode from the skin was 20 centimetres. No filter was used to protect the skin from the softer rays in the earlier exposures, but later on I used felt, and at present I am employing thick cow-hide (sole leather) for this purpose.

On February 20th the lower and upper anterior areas were irradiated separately, each getting an exposure of 15 minutes. No constitutional disturbance having occurred after these exposures, save a slight rise in temperature, to which I shall refer later, on February 26th an exposure of 15 minutes was again given over the left epigastric region. An examination of the blood on March 1 is given below (*vide* Table II.).

It will be noticed that there is an increase in the number of white cells present. This phenomenon has been noted at the beginning of X-ray treatment in leukæmia (1).

On March 4th a 15 minutes' dose was given over the " mid-axillary area."

On March 6th, 18 minutes over the " lower anterior area."

On March 8th, 18 minutes over the " upper anterior area."

On March 11th, the same exposure over the " left epigastric area."

March 16th, the same over the " mid-axillary area."

19th, the same over the " lower anterior area."

23rd, the same over the " upper anterior area."

The patient had now, within thirty-one days, received ten X-ray exposures—4 of 15 and 6 of 18 minutes duration—in the

manner I have described. She developed no unpleasant symptoms of any kind, and she had no dermatitis. Considering the vigorous X-ray treatment she had received this was to me very encouraging, more especially as her general condition began to improve. Her statement that she was feeling much better was verified by the report from the Pathological Laboratory, which gave her blood count as given below (Table III., March 23rd).

On March 30th she had an exposure of 18 minutes over the "left epigastric area."

On April 2nd there was well-marked pigmentation over all the exposed parts, more especially over the "mid-axillary area," which was irradiated on 4th and 16th (15 minutes each), and, lest I might cause a dermatitis, I deferred irradiating this area for a little while longer, so I exposed the surface corresponding in size immediately behind it, when I gave 18 minutes. In a day or two a slight erythema developed over the four named areas, which lasted a few days, but caused no inconvenience to the patient, nevertheless I considered it safer to discontinue X-ray treatment for the present.

On the 5th of April her blood count was as given below (*vide* Table IV.).

Despite the fact that irradiation was now suspended her condition continued to improve, as will be seen from the report appended, which was on a specimen of blood examined on April 24th (*vide* Table V.).

On April 27th the spleen was very much reduced in size, and her condition was much improved, and all traces of dermatitis had disappeared; I again gave a 20 minutes' dose over the lowest part of the spleen in front, but protecting the skin by means of felt—a procedure first suggested by Dr. Ironside Bruce, of Charing Cross Hospital (2).

On May 4th she got an exposure of 21 minutes over the "left epigastric area."

May 7th, 20 minutes to "mid-axillary area."

May 15th, 20 minutes to "upper anterior area."

May 18th, 20 minutes, "left epigastric area."

The patient now left hospital, and the improvement noted in March had continued, and her spleen was considerably smaller, extending only for about $2\frac{1}{2}$ inches below the left costal arch.

As directed, she returned for treatment on June 20th, when she looked well and had put on flesh. Her spleen continued to reduce in size, and now it was not quite an inch below the rib cartilages. It had also receded from the anterior abdominal wall, and, as it was freely movable, it could be pushed up beneath the margin of the lower ribs.

The condition of her blood on June 21st is given below (*vide* Table VI.).

On June 23rd she received 20 minutes exposure, and she went home to the country the following day.

On November 11th she again returned for treatment, and she declared that during July and August she felt that nothing whatever was the matter with her. In October she had slight discomfort in her left side, and during the first ten days in November she at times felt a slight inclination to vomit. This gastric disturbance has now passed off, but her appetite is less. Her spleen is larger than when she left hospital in June, but it is freely movable. The fingers can be pressed into the abdomen above it, and it can be pressed up beneath the ribs, leaving about 3 inches of its anterior surface to be palpated below the costal arch.

An examination of her blood on November 15th is given below (*vide* Table VII.).

On November 11th she had an X-ray exposure of 20 minutes over the lower and anterior surface of spleen in the umbilical region, with thick leather protecting the skin.

On November 15th she had an exposure of the same duration over the upper and inner surfaces of the spleen.

On 18th, 20 minutes over an area immediately external to the part last exposed.

As menstruation came on on the 21st inst. treatment was discontinued until the 25th, when she had an exposure of 20 minutes over the "mid-axillary area."

Summary.—Since February 20th she received twenty-two exposures over the splenic area, the average duration of each being 18½ minutes. Her red blood cells had increased from 3,100,000 on February 15th to 5,072,000 on June 21st; they dropped again, in the absence of treatment, in July, August, September and October, to 3,488,000 on November 15th. The

percentage of hæmoglobin increased from 60 per cent. on February 15th to 75 per cent. on June 21st, and to 77 per cent. on November 11th; the white cells diminished from 566,000 on February 15th to 36,000 on June 21st, and had increased again to 168,750 on November 11th.

The diminution in white corpuscles was chiefly in the mononuclear cells—the polynuclear cells increasing from 32.7 per cent. to 60 per cent., and the nucleated reds disappearing, as compared with the differential count first made on February 15th.

There were never any ill effects from X-ray treatment, save the very slight erythema which appeared in April; she at no time suffered from any constitutional disturbance as the result of irradiation, but I noted that for a day or two after each exposure she had a slight rise in temperature, but this never amounted to more than half a degree.

A pyrexial condition associated with severe constitutional disturbance has been frequently noted in treating these cases, and it has been attributed to the circulation of toxins in the blood due to the disintegration of large numbers of white cells.

The division of the superficial area of the enlarged spleen into several spaces, and irradiating each space so marked out in turn at short intervals, was an experiment, and as it worked well in this case I intend to follow it in the future. It has the advantage that the spleen itself can be exposed to the cross-fire of the X-rays from various angles, so that the skin in any one part will get only a fractional amount of the entire irradiation, thereby diminishing very appreciably the risk of causing a dermatitis and increasing the quantity of X-rays applied to the underlying spleen.

The prognosis of leukæmia when treated by the X-rays is perhaps better than with other therapeutic agents.

Radiotherapy is more costly than other methods of treatment and not dangerous when carefully and

judiciously applied, and the response to it is certainly rapid. There is a sense of well-being from the earliest stages of the treatment which is scarcely attributable to the mental effect on the patient of the elaborate apparatus which must be employed. Relief from the distressing symptoms is obtained very early, but the outlook as regards absolute cure is still gloomy. In the literature on the subject few, if any, permanent cures are recorded—recurrences on the suspension of treatment are the rule, and this case is not an exception. Even some recurrent cases have not responded to resumed and active X-ray treatment.

In conclusion I must express my thanks to Dr. Farnan for placing his notes of the patient's case at my disposal, as well as for permitting me to bring this case before this Academy, and I have pleasure also in expressing my indebtedness to Professor McWeeney and Dr. W. D. O'Kelly for their exhaustive reports on the different blood-films.

TABLE I.—15*th February*, 1909.

Reds, 3,100,000.
Hæmoglobin, 60 per cent. Reds to Whites, 5.5.1.
Whites, 566,000.

Differential count—	per cent.
Polymorphonuclear	32.7
Transit. Polymorphs.	14.5
Myelocytes (Neutr.)	20.0
Myelocytes (Eosinophile)	0.9
Broken cells	5.4
Hyaline cells	7.2
Large Lymphocytes	9.9
Small Lymphocytes	0.9
Mast cells	9.9
Normoblasts seen	1.0.

Table II.—1*st March*, 1909.
Whites, 704,400.

In this case the leucocytes were too numerous and too varied to make a differential count with accuracy.

General Statement of Leucocytes and Red Cells in Film Preparations.—A large number of megaloblasts are seen, often with nucleus partly extruded. An occasional normoblast with dividing nucleus is seen. The majority of the leucocytes are myelocytes, every variety being represented. The commonest form is a very large cell with minute, scarcely visible, basophilic granulation, or perhaps devoid of granulations, but with a more deeply basophilic ring at periphery of protoplasm. Next to these in frequency come moderate-sized mast cells with very large, often angular, granules, and a nucleus which hardly stains at all. Myelocytes with undoubted neutrophilic granules are not nearly so numerous, and eosinophilic myelocytes, although much more frequent than on last occasion, are decidedly the least common. Polynuclears make up the vast majority of the ordinary leucocytes, whilst lymphocytes seem to be absent. In addition to the polynuclears the ordinary leucocytes are made up of vast numbers of mast cells, and a smaller number of eosinophiles. Punctate basophilia is common in the red cells.

Table III.—23*rd March*, 1909.

Reds, 3,528,000.
Hæmoglobin, 63 per cent.
Whites, 209,333.

Differential count—	per cent.
Polymorphonuclears	46
Broken-down cells	17
Transitional cells	17
Myelocytes (Neutrophiles)	10
Hyaline cells	3
Lymphocytes	2
Eosinophiles	2
Mast cells	1

Megaloblast, 1 in every 100 white cells counted.
Normoblast ,, ,, ,,

TABLE IV.—*5th April*, 1909.

Reds, 4,368,000.
Hæmoglobin, 58 per cent.
Whites, 197,000.

Differential count—	per cent.
Polymorphonuclears	42.0
Transitional cells	17.0
Myelocytes (Neutr.)	17.0
Eosinophiles	7.2
Mast cells	6.0
Lymphocytes	5.0
Hyaline cells	3.2
Broken down cells	2.2
Myelocytes (Eosinophiles)	0.4.

Three normoblasts were seen during the count. The lymphocytes were of the large or medium-sized variety. There were cells with coarse granules, partly basic and partly eosinophilic. These were included with the mast cells. Only those cells with granules of undoubted eosinophilic tinge were classed as eosinophiles.

TABLE V.—*24th April*, 1909.

Reds, 4,224,000. One normoblast seen. Some micro- and megalocytes present. Basophilic degeneration was also seen.
Hæmoglobin, 63 per cent.
Whites, 172,000.

Differential count—	per cent.
Polynuclears	40.0
Transitional cells	19.0
Lymphocytes (large)	11.3
Myelocytes (Neutr.)	7.3
Broken-down cells	7.3
Mast cells	6.6
Eosinophiles	3.3
Parent cells (Schleip)	2.0
Hyaline cells	2.0
Lymphocytes (small)	0.6.

N.B.—For the differential counts Leishman's stain was used throughout.

Table VI.—21*st June*, 1909.

Reds, 5,072,000. No nucleated reds were seen. Some anisocytosis was present.
Hæmoglobin, 75 per cent.
Whites, 36,000.

Differential count—	per cent.
Polymorphonuclears	60
[a] Lymphocytes (small)	10
Transitional cells	9
Neutrophile Myelocytes	8
Mast cells	8
Lymphocytes (large)	4
Eosinophile Myelocytes	1.

Table VII.—15*th November*, 1909.

Reds, 3,488,000. Some anisocytosis, no "blasts," seen.
Hæmoglobin, 77 per cent.
Whites, 168,750.

Differential count—	per cent.
Polymorphs.	53.3
Transit.	6.6
Large lymphos.	6.6
Small ,,	2.8
Eosinophiles	2.3
Mast cells	10.0
Hyaline	2.8
Neutr. Myelocytes	14.3
Eosinophile ,,	0.4.

Note.—Patient returned to hospital on March 4th, 1910. She was quite well till February 1st, when the spleen again began to enlarge. On examination the spleen was very hard, and it filled the left side of abdomen, and extended beyond the middle line towards the right iliac fossa. Patient was very anæmic, and complained of severe pains in her legs and back, and her

[a] Compare previous counts.

breathing was much distressed. Despite regular and active irradiation she got gradually worse, and died on her way home April 4th, 1910.

REFERENCES.

(1) Medical Electricity. Lewis Jones. P. 367.
(2) Proceedings of Royal Society of Medicine. February, 1908.

Dr. W. G. Smith said that for himself, and he thought he was speaking also for his colleagues, he and they welcomed any treatment, even an empirical one, that promised to be of service in dealing with the disease. They had, however, to take into account the clinical fact that the disease was liable to very curious remissions, which would render the judgment of therapeutic success all the more difficult.

Dr. Watson said he had treated five or six similar cases, and had generally exposed the spleen in six or seven areas. In his experience the treatment had not shown itself to be of very great value.

Dr. McWeeney said he had been immensely surprised, after having seen the state of the woman's blood in February, to see her the other day in such wonderfully good health. He examined her blood and noticed how very well formed the leucocytes were, and he could not find a single normoblast. Whether the change was only the partial recovery that one met with in such cases it was impossible to say. It was essentially a disease of the marrow, and how could they expect to benefit it by radiating the spleen?

Dr. W. G. Harvey said the radiating of the bone-marrow had been discussed and tried, and in no case had it been anything like as successful as radiating the spleen. It had been suggested some years ago that in the destruction of the lymphocytes and leucocytes which followed the radiating of the spleen there was a leucotoxin set free, which acted on the pathological cells formed. There was no doubt that the X-rays in general had a tendency to destroy cells of a pathological nature rather than normal cells, and it did not seem impossible that, once the process had commenced, bodies might be set free which could continue to have the same action on remote parts of the body.

Dr. C. M. Benson said he had found that by treating the various parts separately dermatitis could be avoided. When a patient

came back after a recurrence, it would be found that the tumour receded very much less quickly than on the first occasion, and in a case of long-standing, he had found it impossible to reduce the spleen at all.

Dr. Parsons quoted a case of his own which was almost identical with Dr. Hayes' case. She was fifty-five years of age, and had had a prolonged course of arsenic without any improvement. The condition of her white cells was almost identical at the end of the treatment with what it had been at the beginning. After the tenth or twelfth application of the X-rays a marked improvement took place in her blood, and the spleen decreased to such an extent that it was almost impossible to palpate it. She went away feeling perfectly well, but returned within a year's time with the spleen increased and a relapse in the condition of the blood. She again received benefit from the rays, but died in six months. It was quite certain that the rays effected a diminution of the spleen, but how it was brought about he did not know. The improvement in the blood was not brought about by the destruction of the white cells, although the rays might inhibit their formation.

Dr. Rowlette said he had performed a *post-mortem* last year on the body of a patient who had died of heart disease while under X-ray treatment. The leukæmia was discovered when she was put to bed in hospital, and as far as it was concerned, she had improved considerably. In the microscopic examination of the spleen he did not find any of the patches which were said to be typical of the disease, and it was possible that the X-rays had caused their disappearance.

Dr. Hayes, in reply, said he believed the woman's present condition to be due altogether to the X-ray treatment. Out of sixty-three cases collected as treated by the rays, only four were alive in from three to six years after the primary symptoms, so that it did not promise very well for the patient.

AN AUTOGRAPH ACCOUNT OF A CASE OF SANE HALLUCINATIONS DUE TO ALCOHOL AND ATROPIN.

By W. R. DAWSON, M.D., F.R.C.P.I.;
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[Read in the Section of Medicine, November 26, 1909.]

The subject of the experiences about to be described is a literary man of great ability and a recognised authority in a certain department of learning, who had unfortunately been addicted to the abuse of alcohol for many years and had decided to try the effects of atropin treatment. He was perfectly sane. Previous to his admission he had been living practically altogether on milk and whisky for a considerable time, taking five or six glasses of the latter in the day. The whisky was at once greatly reduced, though not altogether stopped; and combined injections of atropin and strychnin were begun on the day of his admission (November 22nd), with dietetic and other treatment; but no sleeping draught was given either that night or the next. This was the first case which I had treated by the atropin method, and I should not now commence the injections so soon after admission. No other case, of a good many since treated, has suffered from hallucinations, but they have often been met with by others.

It may be said at once that, with the exception of these experiences, the treatment was quite successful for over four months, the patient being weaned from alcohol without much discomfort and losing all taste for it. At the end of this time he received a severe shock in the

sudden death of a near relative, whereupon the craving returned in force, and he lapsed again and derived no benefit from subsequent courses of atropin treatment. He treated himself elsewhere by the "Normyl" method, however, and has now been perfectly well and a total abstainer for nearly four years.

The following is his account of his experiences:—

"November 24th.—The first I saw of them was the brown man. He was leaning over my bed at arm's length; I could easily have touched him. He was a tall, dark man with brown eyes and beard, and he looked very grave. I was wide awake and took in every detail of his dress and expression, though it was night and the room was dark. His appearance surprised me very slightly, for I had noticed him walking about close by in the grounds outside my window, which was on the ground floor. I thought he had mistaken his bedroom, and said so. I had no suspicion that he was any other than the man I had seen walking in the afternoon. I fancied that he explained that he had come to see how I was getting on, and then he slowly backed towards the door and left the room. I noticed that he did not seem to open the door, but simply went through it. Then for the first time it flashed upon me that he might not be a real man, and I remember that I was not exactly frightened but felt a little uncomfortable. I was not perfectly sure about the closed door; I might have been mistaken. I remained absolutely awake and alert. I did not light my candle. I was wondering if the brown man would reappear or come back, supposing he was really wandering about looking for his room.

"The pale girl completely set at rest all doubts. She appeared soon after the brown man had left, but she did not come in at the door, but suddenly stood at the foot

of my bed and gazed upon me. I had known her for years, and she was a close friend, but there was no smile of recognition on her face. She, too, was grave and sad-looking. She seemed offended, and refused to shake hands. She stood there for some minutes as it seemed. I realised almost at once that she was not flesh and blood, for I knew that she was fully a hundred miles away and was herself laid up, slowly convalescent after a tedious illness. By no possibility could she be at my bedside in that house in the middle of the night. She vanished as suddenly as she came. I went to sleep, but presently waking up I saw her again beside me—at least I saw her head on my pillow not six inches from my cheek. I was not frightened; I was glad to see her; her face was too familiar to me to be anything but a happy sight, but the gravity and distance of her expression discouraged any approach. I knew it was not she really, and I waited and watched her in silence. Presently her head was gone, and after some time, spent in wondering if she would return, and also whether I was going to be haunted by such visions, and whether I should end in terror, I fell asleep. I slept well, without dreams.

"In the morning my visions were as real to me as if they were present. They are just as real now, after many months, though I have never seen the brown man or the pale girl as visions again, while often meeting both of them in the flesh. The passage of months has not dimmed the vividness of any of my hallucinations. When the doctor came in on his rounds that morning I described to him what I had seen. I should explain that I was in a medical home and was being treated for the alcoholic habit and its effects. An essential part of the treatment was the hypodermic injection of a special drug, and this injection had been begun two days before.

The drug was not in the nature of a sleeping draught, and was not injected near bedtime, nor had I been given a sleeping draught on the previous night or at any time for years past. The doctor, who is a mental expert, did not appear surprised at my account of my experiences. He said the special drug he was 'exhibiting' had been known to produce hallucinations, especially in certain conditions of the patient's health, and he believed these delusions would pass off. I explained that my only anxiety was about my mental state. I was not afraid of seeing things, but I was undoubtedly afraid that the phenomena might indicate some permanent mental disease. On this point he firmly reassured me. He regarded the hallucinations as purely temporary and due to temporary causes.

"Satisfied with this assurance I did not worry about the subject that morning. I am positive that I was not on the look-out for further developments. I went for a turn in the private grounds in company with one of the young doctors. As I walked down the avenue I noticed a cart-horse lying in a peculiar attitude in a ditch beside the road. I now know that there is no ditch there. I also saw a cart and horse going across the field. No cart was out there on that day, I was afterwards told. When we went into the garden I saw a spaniel coming towards me from a side path, and asked my companion whether he saw any dog. He said there was none. All these outdoor sights I took to be natural until assured that they were not. It was a bright sunny day, though the season was November, and in the afternoon I stood at the closed window looking out upon the wide lawn. On such a lovely day it was no surprise to me to see that numbers of people—patients, I supposed—were enjoying themselves in the sunshine. There were crowds on the lawn

and on the broad path that ran between it and my window. On the lawn itself women dressed in a very fantastic fashion were dancing wildly in the joy of the day. A carriage drove up on the path, with people in it—visitors, as I imagined, come to see their friends among the patients. The crowd on the lawn capered down gleefully to welcome the visitors and clustered in a sort of ditch which lay beyond the path. They all had very queer expressions on their faces, but this did not astonish me, because I was quite aware that the home was a large place full of all varieties of mental cases, and I was quite prepared to see mad people disporting themselves in the grounds. It never occurred to me that no doctor would allow male and female lunatics in an acute stage of dementia to frolic about together, and as I had never been in the patients' grounds I was not aware that there was no road by which a carriage could get to where I saw it, and no door or steps whatever on the left of my window. The people were quite closely packed on the steps leading up to a door (as I supposed) on the left of my window. I simply took the whole thing as an amusing, yet rather horrible, scene in the grounds of a 'lunatic asylum,' and was glad that a stout line of iron palings separated the crowd from the narrow grass plot outside my window.

"But it did not keep off their *eyes*. I shall never forget those eyes, so preternaturally grave and melancholy, each and all fixed in a relentless, immovable stare upon me. I was looking at them from behind a muslin curtain, but they saw me clearly through it, and never took those sad eyes off me for a moment. The only pair of eyes that seemed to me to show any expression that was not profoundly grave belonged to a tall, good-looking girl, dressed in black silk and a large picture-hat, and carry-

ing a parasol, who stood quite near me on the stone steps, and she certainly had a bold, challenging look that was comparatively cheerful. The other eyes all wore the same look of unearthly gravity with a curiously unpleasant depth of furtive cunning which struck me as characteristically mad. I watched them and they all watched me for a considerable time, till at last I became oppressed by their unblinking gaze and went back to my armchair by the fire and resumed my novel. I did not feel at all 'creepy,' for I had not the least suspicion that what I had seen was not real. I knew nothing about the other patients or their habits, and though I wondered at their being allowed to disport themselves so freely, with apparently no keepers, I ascribed their unwelcome observation of me to a natural curiosity about a new 'inmate.' I can honestly state that I had no idea that I was looking at phantasms of my own mind.

"I remained sitting back to the window reading for a couple of hours, without disturbing myself to look out; but when at last I turned round I was annoyed to see five people standing at my window with their faces pressed to the glass, staring at me with all their might. I shall not forget those faces in a hurry: they were distinctly malicious. There was near the middle a round-faced, rosy, fat little man with white hair and a jovial look, contradicted by the predatory cunning of his eyes, which seemed to want to get at me. Next him was a slatternly girl with red hair, badly in need of a brush, who seemed very eager to get in, and made disagreeable faces. She had beside her an evil-looking old hag, with the most malicious eyes I have ever seen. A tall man stood at the end next her, and at the other end, next the fat old fellow, was an undersized, unshaven type of mechanic who followed my every movement with in-

quisitive eyes. They all took an immense interest in me, and jumped up and down as if they were raising themselves on tip-toe. They seemed to be standing precariously on some narrow ledge in the wall and holding on to the window-sill. Sometimes she raised an arm and pointed to me. I heard no voices, but their mouths moved as if speaking. I concluded that some of the people in the grounds, noticing my withdrawal from the window, had pushed forward and climbed the railings to see where I had gone. I still believed them to be real lunatics, and their closeness made me uncomfortable.

"There was no bell in the room, and I did not want to make a fuss by going out in search of the doctor; but fortunately a servant came in to make up my fire, and I drew his attention to the nuisance these people were making. He took a quick glance at the window, and gave an odd grunt which displeased me. I said sharply that I would not allow this spying into my room, and he said he would send the people away. I heard him go out into the grounds by a door (which I afterwards found did not exist) in the passage, and order them away. They went at once, but came back almost immediately, and this time they became aggressive. The slatternly girl pushed up the sash, and, putting her bare arms through the bars, lifted up my looking-glass from the toilet-table and began to draw it gently towards her, looking at me all the time with quick, furtive glances. I did not like it at all, and called for the servant and bade him fetch one of the doctors and stop the nuisance. A young doctor came, and I showed him the girl in the act of pulling the looking-glass through the bars. I remember well the shock it was to me when he patted me kindly on the shoulder and bade me not to worry about it, saying that there was no girl nor any other person at

the window, but I should soon be all right. Up to that time I had, curiously, never connected these visions with the confessed hallucinations of the preceding night: I had never suspected that the faces at the window were unreal. Now at last I recognised the fact that they were phantasms of my own brain, and the fact disturbed me much more than the belief that I was being bothered by inquisitive lunatics. I feared for my own sanity.

"After the young doctor had gone—and with him the figures at the window—I settled down again, hoping that the illusion was over. But on turning round in my chair I saw that the mechanic had come back alone and was staring at me as before. After bearing it for a time I got up and pulled down the blind (my lamp was already lit), but whenever I turned round there was the mechanic's eye peering at me through the narrow slit between the blind and the window-frame. I compelled myself at last to abstain from looking at him, but I was perfectly conscious of his gaze.

"Presently I forgot all about him. My attention was suddenly diverted to a new appearance. Scarcely more than a yard from my chair, on the left, stood two little mannikins, about two feet high, very neatly dressed in black velvet, as I remember. They looked like well-behaved twins just dressed and brushed for going to church. They both regarded me steadily with the same curious inquisitive look which I had noticed in all the others. They evidently thought me a strange creature, much as the Lilliputians regarded Gulliver. I looked at them and they looked at me for some time. Then I got up and made for them, and they vanished; but on turning round to go back to my chair I found that they had apparently dived under the table, for there they stood at the other side—the right—of my chair, using a port-

manteau in the corner, next the wall, as a convenient platform. I let them stand, immovable, side by side, grave and inquisitive as ever. I did not speak, and they did nothing but look. They rather amused me, they were so natty and gentleman-like in their ways. At last they seemed to bow me a sort of farewell—with their eyes only—and politely backed towards the door. I saw one of them lift his little arm and turn the handle. The door opened, and they were gone.

"But they left their representatives, and I did not admire the exchange. Just in the spot where they had first stood, beside the table, were two monkeys of the usual organ-grinder kind. They squatted there and cracked nuts noisily (this was one of the few *sounds* I heard throughout these experiences) in their teeth. Presently they vanished from the spot and instantly re-appeared at the other side of my chair, sitting on the portmanteau in the corner. There was one agreeable distinction about these monkeys: they did not stare at me with the horrible gravity of the humans, but their little eyes shifted perpetually like ordinary monkeys' eyes, and though sad, that after all is characteristic of the whole tribe. Still, I did not like the presence of these little beasts, and after a time they so oppressed me that I got up and left the room in search of company. There was a doctor's study about twenty yards down the passage, and there I went. On the way I had to pass the open door of a housemaid's office, which was unlighted, and there close to the door I saw a figure leaning as if on the watch. Further on was a stove, and in front of it crouched a woman apparently putting on coal. I was perfectly aware that no such persons really existed. When I got to the doctor's study I found it empty: he was away with some patient. The blinds of the two

windows were up, and there to my disgust I saw my five friends again—the fat old man, the slattern, the old hag, mechanic, and the tall fellow—all jumping up and down and pressing their faces to the glass to have a good look at me. There was a kind of devilish glee about their expression and attitude which was uncomfortable. They were evidently triumphant over my annoyance at this unexpected *rencontre*. When I turned my eyes away from them to glance at the other window, there they were at once. I had clearly not improved matters by my change of room, so I went back to my own, where I found the monkeys still in occupation. The doctor came in during the evening, and I told him what I had seen and still saw, for the monkeys remained munching their nuts all the time he was there. This was not usually the case, as I found in subsequent hallucinations. As a rule the entrance of anybody into the room was the signal for the instant disappearance of all my *other* visitors. In the same way a sudden movement or 'pounce' on my own part would generally put all apparitions to flight.

"I do not remember that I saw anything after going to bed. I had a sleeping-draught. The next morning (25th) I was perfectly clear in my head; but in walking in the grounds the animal hallucinations recurred, and towards night matters became decidedly more interesting. My friends the monkeys reappeared, and presently the corner of the room became the scene of a number of incidents. First a whist-party was got up, with my portmanteau for a table. I noted especially an old man with a long beard, and an old lady, a diminutive Mrs. Battle, intensely absorbed in the game. So were all the four tiny players, and when I leaned over to watch the play—I could see the cards plainly—they were obviously

annoyed, and the old gentleman looked daggers at me. At the same time a girl made herself very comfortable in a sort of cot against the wall next to the whist-party. She could not have been more than three feet long, but her head seemed of the normal size, and she had a decidedly unpleasant, not to say wanton, expression. Her cot must have been composed of some newspapers thrown aside in the corner, and I have a recollection that they seemed to rustle under her movements, for she was continually curling herself up and then uncurling. I liked her less than any of my previous visitors; but another soon appeared, who struck me as equally uncanny. This was a precocious imp of a baby, who sat on the foot of my bed and grinned impudently at me. I was so outraged by this that I flung a stick at him, whereat he vanished from the top of the bed, but instantly reappeared, making faces at me from under the end of the valance. I rattled my stick under the bed, and forthwith there he was on my pillow. I began to think I would try company again, and set out for the young doctor's study. I was the more inclined to do so because just as I was rising from my chair to go a horse's head slowly reared itself behind my washstand (which stood flat against the wall, with no space between), and craning over began to drink the soapy water. This was a little too much for me, and I made for the study, passing again the figure by the door and the woman crouching before the fire. However, I found the study empty of doctors, but filled with a large party of spectral guests, ladies and gentlemen, in evening dress, playing cards, or chatting (though I heard no voices), or carrying on what appeared to be pronounced flirtations. I went back past the woman at the stove and the other dark figure. I did not like passing them, but I would not

shirk it: without being exactly frightened I felt 'creepy,' and had to hold myself pretty tightly in hand.

"That night the doctor proposed that I should have a man to sleep in my room in case I was disturbed, and after some demur—for I hated to show the white feather—I agreed. As it turned out, the man's company was a decided relief. It was indeed a remarkably lively night. I remained wide awake long after the man had gone to sleep. There was a bright fire, and I could see every part of the room. The first thing that happened was the slow opening of the door. It opened only a little, just enough to let a person through, yet at first no one came through. Then I became aware of an eye reconnoitering the room through the crack at the hinges of the door, and, oddly enough, the door was hung on the wrong side; it really opened on the left, but that night it opened on the right, the eye peeped through by the hinges on the left. Apparently satisfied by its inspection the eye was withdrawn, and then five or six men crept stealthily into the room, glancing suspiciously at my bed, and then, seeing me apparently asleep (for I had nearly closed my eyes, to give my visitors fair play, and was watching them through my lashes), they all went to the fireplace. The fire evidently needed making up, for they moved a chair or two close up to it, sat down, and began shovelling on coals. Two of them came up to my bed; one sat at the foot, and the other leaned over me, tapped me on the chest, and carefully examined my eyes to make sure that I was asleep. I confess that when he bent down I kept my eyes shut tight. Then they all grouped themselves round the fire, and I heard them whispering. They were picturesque fellows, typical stage bandits, in velvet jackets, and shorts, and silver buckles, and all the rest. They had a proper truculent air which compelled me to

lie as still as a mouse. Presently a couple of them deliberately sat down on my attendant's pillow. I called to him for fear he should be hurt—it is clear that at this moment I was not able to distinguish the hallucinatory character of the appearances; he woke up and said there was no one in the room. The bandits then crept away to the door in a gliding, stealthy movement, quite in the 'Surrey-side' manner, and went out, to my unfeigned relief.

"Hardly had they gone, however, when a very solemn man with a long brown beard seated himself beside my bed. The bed was close against the wall, yet he was seated on the wall side as though in a niche. I could only see his torso, and as he was not two feet from my face I wondered where he put his legs. I spoke to him and told him to go away, quite politely. I did not wish to make him angry. He neither moved nor spoke, and unlike the others, never looked at me but stared fixedly before him. The attendant woke up, hearing my voice, and assured me that there was no one there. But there he was all the same, the long-bearded man, and it was an infinite time before he vanished. It was a good many days before I could go to sleep without glancing at the place where the 'niche' ought to have been to satisfy myself that my friend was not there.

"I suppose I must have slept after that, and when I woke it was about 7 a.m. and the attendant had already got up and gone. His absence was amply compensated by a perfect *levée* of early visitors. First I noticed a little girl in a black dress and very wild, frightened eyes. She went up to the corner of the room where my wash-stand was. It was (apparently) not flat against the wall as it had been when the horse drank out of the basin, but stood corner-wise, and there was a much bigger dressing-

glass over it than really belonged to it. The little girl stood and admired herself frankly before the glass; then she deliberately undid her black frock and slipped it down to the ground and stepped out of it, just like any other girl, and then proceeded to put on a pale green frock. When this was done she ogled herself delightedly and posed before the glass. Then she brushed her long yellow hair carefully—with my brushes, a man's brushes, not a woman's handled brushes. I saw it all clearly, but if I moved and got up on my elbow to observe her, she instantly darted a frightened look at me. At last I got out of bed and went to investigate her, and found only my towel-horse and the toilet-table and washstand in their usual places against the wall, undisturbed (I did this more than once). As soon as I returned to bed, however, she came back to the glass, and there she remained until the arrival of the doctor more than an hour later.

"Meanwhile my room had become densely populated. At the foot of my bed stood a chest of drawers; beyond that was the portmanteau already referred to, in the extreme corner. People began to come in gently from the corner where the portmanteau was. An old woman and her daughter (apparently), the latter nursing a baby, came slowly and furtively and sat down upon a couple of (quite imaginary) chairs in front of the chest of drawers. Several men followed them. One sat on the chest of drawers; another stood between it and my bed and distinctly glowered at me—he had a very forbidding, dark visage. Others leaned against the chest of drawers, and among them was a fair man with light hair and a weak face with whom I became quite intimate. I used to arrange the bedclothes so as to intercept my view of the glowering man as far as possible, but I rather liked the appearance of the fair man: he looked so good-natured.

I talked to him, asked him why he was not at work at this time of day—for he had the air of a carpenter—and remonstrated with him on the vice of idleness. I chaffed him about his melancholy air, and looking into his eyes I even succeeded in making them *smile* repeatedly. He was the only phantasm on whose expression I had any influence. When I talked to him, the others looked at me with keen interest. The women crept forward as if to listen, but I waved them back. As usual, any sudden movement or leap out of bed sent the whole crowd packing at once, only to return one by one, very stealthily, after I had gone back to bed again. Two cats came up close to me on the floor. I lighted a match from the box by my side and singed the whiskers of one of them. It started and shook its head, and both cats kept out of arm's reach. I was still chatting to the fair man when the doctor and his assistants came in on their rounds. On their entrance the men and women and cats and the little girl at the looking-glass precipitately vanished. The doctor and I conversed perfectly normally. I was quite rational and clear-headed, and we laughed together over my queer hallucinations.

"I am not clear about the rest of that day or the following night; but I am strongly under the impression that the brigands came back at night—I recognised them perfectly—and that my morning visitors did not forsake me, including the nursing woman and her mother, and that I chatted again with the fair man, and grew quite interested in him. After that the doctor thought I had had enough of such experiences. He had already stopped the hypodermic drug (on the evening of the 24th), and now he gave me a dose of morphia; and as that did not send me to sleep he gave me a second dose. I had a wild night, and saw and did all sorts of

extraordinary things: but the effects of morphia are well known and have nothing to do with the hallucinations here recorded, which occurred when my mind was unclouded. After a heavy sleep I woke untroubled, and have never experienced the slightest repetition of the hallucinations I have described, though the same drug was repeated in three courses extending over many weeks.

"There remained, however, a curious optical perversity which may have some medical interest. The first peculiarity I noticed, about the time when the human and animal hallucinations appeared, was the funny way in which my ring persisted in working itself up to the middle joint of my finger, and then slowly working back to its usual place. I showed this in all seriousness to the doctor, but he could not detect it. I could only repeat with conviction, *eppur se muove*. One night I asked the attendant why there were fleas in the room. I had certainly brought none with me, and he maintained that none were known in the room, yet there they were, crawling up from under my shirt-cuff, and insinuating themselves between my fingers. I killed hundreds of them, and showed the attendant one which I had smashed upon a piece of paper—where he admitted there undoubtedly was a stain, but equally certainly no flea. This illusion was not repeated, but something on a larger scale interested me a day or two later. I was talking to a friend, and I asked him to notice how the newspaper was heaving up and down, as though something were moving under it. He lifted it up and saw nothing; but *I* saw a number of big grasshoppers wriggling about, and I saw more of them in a medicine glass that was on the table. I said nothing, as I did not know whether he had heard of my hallucinations, and I did not care to discuss this species of aftermath, which,

as a matter of fact, did not recur. Once (December 3rd) I tried to pick up an imaginary playing-card from the table. The only thing that remained, and this lasted for weeks, was a singular appearance of what looked exactly like straws. I saw these continuously in the red of the fire; I saw them in the glow of my pipe where I lighted it; and I saw them forming a criss-cross on the wall-paper, and they continually moved. This was the last enemy to be overcome, and it was very persistent, and had not entirely vanished by January 29th. My eyes at the time were causing me trouble, but no pain. I saw everything in a haze, and could not read even large print or music. My chief resource of an evening was playing 'Patience,' and I found that I often made mistakes, especially in the *red* cards, by not distinguishing a pip. For instance, I was constantly mistaking the three of diamonds for the two, and the seven for the six. I believe the doctors recognise this as a definite ophthalmic disease, and connect it with smoking; but as I have been an inveterate smoker for a score of years, and never experienced this confusion before or since, I am forced to believe that there must have been some ancillary cause. I am smoking as much as ever, and my sight, though always myopic, is remarkably strong and distinct.

"The main interest in the hallucinations I have described appears to me to consist in two points. First, they were *conscious* hallucinations. Except in the first appearance of the brown man (who might have strayed into my room) and the out-door sights which I took to be real at first, and once or twice at night time, when my mind seemed to be a little confused, I knew perfectly well that I was looking at things which were not real; and whilst looking at visions which I knew to be unreal, I talked rationally to friends or turned to my book without

any abrupt transition or interval of recovery. There was no 'trance.' I never experienced anything approaching to panic fear, although some of my hallucinations were calculated to make anyone 'creepy'; and the doctor said that in all his wide experience he had never come across any one who 'kept his head' so thoroughly and went through such trying mental experiences in such a spirit of almost scientific observation. The second point is that, after the first appearances of the brown man and the pale girl, no single face of the score and more that appeared to me was familiar. I never saw one of those faces in all my life: of that I am positively certain. *How*, then, did I create them? or *did* I create them? I am not prepared with any speculations about the 'subliminal consciousness' or 'reminiscences of pre-existence,' though I think my experiences will interest those who have made a study of the late Mr. F. W. Myers's 'Human Personality.' I simply record truthfully, and without exaggeration, precisely what I saw and how I regarded these visions. I may add, to prevent misconceptions, that I am not in the least which is called 'psychic,' never attended a 'spiritist' *séance* or met a medium, and never even thought I saw a ghost. My doctor's *testamur* precludes all suspicion of either fiction or insanity."

The value of the description, apart from the vividness due to the patient's literary skill, lies in the circumstances that the experiences were observed by a man of high intelligence, that he was keenly interested in them, and that he was mentally sound throughout (except while under the influence of morphin given as a sedative and antidote, which rendered him delirious on one evening and night that of November 25th). The fact that the

description was not written until many months afterwards may be held to discount its worth to a certain extent, but the writer has an excellent memory, and comparison of his statements with my own notes taken at the time, as well as the recollections of my assistant and myself, show that his account is substantially accurate. The incidents are sometimes, naturally, related a little more fully than before, a few noted by me at the time are missing, and the experiences are spread over a day longer than they actually occupied—the latter slip being due, no doubt, to the confusion induced by the morphin and its results. An instance of omission is the fact that on December 6th, some ten days after the hallucinations had almost entirely ceased, the patient, while out walking, saw crowds of children, horses and carts, which were not there, and also heard people talking, all of which he has forgotten. But the details which he gives are essentially correct, and the description honestly portrays his impressions of an interesting, if unpleasant, experience. His account of the effect produced on his mental state is also, to the best of my judgment, accurate.

As regards the hallucinations themselves, it may be noted that, notwithstanding the extraordinary realism of the phenomena, there was something unnatural about them in nearly every instance, which, no doubt, would have led to their detection as phantasms had the patient not been in a hospital where he knew that mental cases were taken. As a matter of fact they often were so recognised before they had ceased to be visible. Their almost entirely visual character follows the rule in sane hallucinations; but in one or two instances there were combined hallucinations (auditory and tactile also). Perhaps the most interesting circumstance, however, is that, according to the patient's statement, he was able to

influence the expression of one of the phantasms by talking to him.

They, therefore, differed greatly in complexity, and it is evident that the simpler ones, such as the appearance of straws, derived their origin from the presence of retro-bulbar neuritis, which was curiously indicated by the disappearance of the central pip of a red card, thus showing a central scotoma for red. These were, therefore, rather of the character of illusions than hallucinations, and the true hallucinations must evidently from their complexity have taken origin at a higher level, perhaps the visual ideational centre in the angular gyrus.

GLYCOSURIA AND GRAVES'S DISEASE.

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[Read in the Section of Medicine, January 7, 1910.]

From time to time the occurrence of glycosuria in Graves's disease has been noted by various observers, but rather as an accidental phenomenon than as a combination which pointed to any remote connection between the two diseases. Of late years, however, it has been observed that the combination was more frequent than had been suspected; not only so, but that in a considerable number of cases of diabetes there was a history of past Graves's disease. These facts suggested that there might be some connection not hitherto thought of between the two.

That little importance was attached to this question is seen by a glance at the standard text-books of a few years ago. Indeed, the most recent additions do not add much. In Hilton Fagge's "Practice of Medicine," 2nd edition, 1888, there is no mention of glycosuria in Graves's disease, nor of Graves's disease associated with diabetes. Osler, in his "Practice of Medicine," 2nd edition, 1895, makes no mention of Graves's disease associated with diabetes, but says that in Graves's disease glycosuria is not infrequent. Saundby, in "Allbutt's System," 1st edition, 1897, mentions as an example of the rarer complications of diabetes that it may be associated with Graves's disease.

Williamson, in Gibson's "Text-book of Medicine," 1901, makes no mention of the combination in his article on diabetes. Sir Lauder Brunton, in the same text-book, in his article on Graves's disease, says "the patients generally belong to neurotic families, in other members of which hysteria, epilepsy, diabetes, and mental diseases have appeared." This in 1901, although in 1874 he described, in "Bartholomew's Hospital Reports," Vol. X., page 253, a fatal case in which the two diseases were combined. It would appear, therefore, that in 1901 he did not attach much importance to the combination. A similar case was recorded by Sir Samuel Wilks in the *Lancet* of the following year—1875. A third case was reported by O'Neill in the *Lancet* of 1878. H. Stern, in the *Journal of the American Medical Association*, 1902, has given a general survey of the subject, and has collected twenty-four cases of combined Graves's disease and diabetes. Twenty-two of these were in women, two in men. The ages varied from eighteen to fifty-nine. He quotes Chvostek as having found alimentary glycosuria in 69 per cent. of his Graves's disease cases. Dr. George Murray, Professor of Systematic Medicine in Victoria University, Manchester, has published an interesting paper on the relationship between these two diseases in the *Clinical Journal* of July 28, 1909, and in it reports four cases which have come under his own observation. The first in a female, aged twenty, who had exophthalmic goître and recovered; four years later she was found to have severe diabetes in March, and died in June. The second, a female, aged twenty-five, had Graves's disease for two years; three years later she developed severe diabetes. Third, a female, aged thirty-eight, had Graves's disease and recovered; she was given thyroid extract, the Graves's disease recurred, and in addition

diabetes appeared. Fourth case, a man aged twenty-two, had Graves's disease as a youth; eleven years later he developed diabetes.

To this valuable paper of Dr. Murray I am indebted for most of my information and ideas on the subject, and I have freely made use of it in this short communication. To my colleague, Dr. Magee Finny, I am indebted for bringing Dr. Murray's paper under my notice.

It would appear, then, that this combination of diseases is not a mere accident, but that there exists some morbid connection between the two. Let us see what further support there is for this idea.

We are only beginning to understand the functions of some of the ductless glands, and also beginning to realise that other duct-bearing glands besides the liver have important internal secretions. It is probable that between these glands there is considerable interdependence, but we know little about this yet. An illustration of it, however, is seen in the case of myxœdema. Here one of the symptoms of the loss of thyroid secretion is the cessation of the secretion of the sweat glands, and probably also of the sebaceous glands. Apparently this is a case of interdependence, for when thyroid extract is supplied in such cases artificially, not only do the other symptoms disappear, but the sweat glands again begin to act. If diminished activity of the thyroid reacts on the activity of certain glands it is quite likely that over-activity of the thyroid will also react on some other gland or glands, and one example of this over-action is again seen in the case of the sweat glands, which in Graves's disease are stimulated, and cause excessive sweating.

In Graves's disease there appears to be excessive activity of the thyroid gland; and of recent years great

interest has been centred round the pancreas as the organ in some way at fault in diabetes.

Of twenty-seven cases of diabetes examined by Saundby, in only six of these could the pancreas be described as normal, and, as he says himself, "we have a good deal still to learn about the significance of slighter changes in its structure, and it is possible that, while appearing normal to the naked eye, it may be extensively diseased." We know that extirpation of the pancreas in dogs is followed by diabetes, and in one case in which it was removed in man (by W. T. Bull) diabetes resulted. In many cases of diabetes a gross lesion of the pancreas has been found, and in many others, in which there was no gross lesion, microscopic changes of structure have been made out. These finer changes are chiefly an increase of fibrous tissue or changes in the so-called islands of Langerhans. Now, it is believed that these islands of Langerhans are concerned with the internal secretion of the pancreas, and that the internal secretion, or, rather, its disturbance, is concerned with diabetes.

Rolleston, discussing cirrhosis of the liver, says:—"The gland (*i.e.*, the pancreas) cells undergo fatty and pigmentary degeneration, but, according to Steinhaus, the islands of Langerhans, which play an important part in the production of the internal secretion and in the production of diabetes, remain intact. In the cirrhosis of hæmochromatosis, however, which is an exception to this rule, the islands of Langerhans are eventually destroyed, and as a result there is diabetes." Further on he says:—"It is rare to find sugar in the urine in ordinary cirrhosis. Glycosuria occurs in a high proportion of the cases of pigmented cirrhosis. . . . but it is then due to a concomitant fibrosis of a very intimate nature in the pancreas (involving the islands of Langer-

hans), a change which is not present in ordinary cirrhosis." And again:—"When the fibrosis of the pancreas reaches such a degree that the islands of Langerhans are involved, diabetes results."

Quite recent researches practically confirm this. Macallum, in the Johns Hopkins' Hospital Bulletin for September, 1909, summarises thus the results of his experiments:—"When a portion of the pancreas is separated from the rest, and the duct ligated, it undergoes extensive atrophy, a tissue remaining which is apparently composed of enlarged islands of Langerhans and the remains of pancreatic ducts. If the rest of the pancreas be removed, this atrophied remnant is capable of warding off glycosuria, even when considerable amounts of dextrose are injected. When it itself is removed also glycosuria appears at once. Whether this glycosuria would persist until the death of the animal remains to be proved. The experiment seems to demonstrate the specific control of carbohydrate metabolism by the islands of Langerhans."

H. Eppinger and others have shown that there is a direct relationship between the thyroid and the pancreas, and that the glands tend to inhibit each other—that as a result of hyperthyroidism there is inhibition of the pancreas causing relative insufficiency of the internal secretion with alimentary glycosuria or its easy induction (*Zeitsch. für klin. Med.*, 66, 1908, p. 1). Lorand has found that thyroidectomy caused increase of Langerhans' islands. Also, that the glycosuria resulting from removal of the pancreas ceased on removal of the thyroid. The animal, however, lived only a short time. (*Comptes Rendus des Séances de la Soc. de Biologie*, t. lvi., p. 488.) Hector MacKenzie says:—"The carbohydrate secretion is influenced to some extent by the thyroid

secretion; sugar is excreted in the urine of dogs after thyroid feeding, when large quantities of carbohydrates have been given; and the amount of glucose which can be given without causing glycosuria is less than the average in patients suffering from Graves's disease or in persons who have been treated with large doses of thyroid substance." In one of Stern's cases the Graves's disease had subsided, but diabetes supervened, and on the administration of thyroid extract with arsenic there was a rapid and large increase of the output of sugar. In one of Dr. George Murray's cases that had recovered from Graves's disease the administration of thyroid extract was accompanied not only by a relapse but also by diabetes.

In view of all these facts, Dr. George Murray suggests that in Graves's disease the excessive thyroid secretion thrown into and circulating in the blood first inhibits the islands of Langerhans, and so causes relative insufficiency of the internal secretion, and thus alimentary glycosuria or its easy induction on increase of carbohydrate food. Finally, it may lead to atrophy of the islands and thus brings about true diabetes.

These very interesting questions have induced me to bring under your notice, and so record, a case which I have lately met with, and which directed my attention to this subject:—

On the 2nd of November, 1909, a young girl, aged eighteen, was admitted to Sir Patrick Dun's Hospital under my care, with symptoms of diabetes. She was greatly emaciated, weighing only 6st. 4lbs. 3ozs., though of average height. She stated that she had noticed herself getting thin for about two months, and latterly had become very weak, but began to suffer from thirst about the middle of August—that is, two and a half months before. She had also missed two menstrual periods. It was

evident that she was also suffering from Graves's disease, there was only slight exophthalmos, but well-marked retraction of the lids; von Graefe's sign was present in limited extent, being only seen when the visual axis passed below the horizontal line. There was decided bronzing of the eyelids. There was slight tremor of the hands, and the skin was unduly moist, though no profuse sweating. The thyroid was moderately enlarged; it pulsated, and a thrill could be felt in it. The heart's action was tumultuous and very rapid, the cardiac impulse being widely diffused; the apex beat violent, but no murmur could be detected. The pulse-rate varied while in hospital from 100 to 140. The areolæ of the breasts were deeply pigmented, but the breasts themselves were small and functionless. She complained of the throbbing of her heart, and said it had troubled her for a long time—several months—before she noticed the thirst or the increased quantity of urine.

While in hospital the amount of urine passed varied from 100 ounces to 177 ounces in the twenty-four hours. Its specific gravity was 1042, and at first it gave a slight diacetic acid reaction with perchloride of iron, but the reaction for acetone itself, though looked for, was not found. The diacetic acid reaction disappeared in a couple of days.

Her carbohydrate food was considerably limited, but not altogether withdrawn, and she was given crude opium in the form of compound soap pill night and morning. She remained in hospital only seven days, as she fretted greatly to get home, and on two occasions got into such an excited hysterical state that more harm than good would have resulted in her remaining longer. Her condition could not be said to have improved in any way during her stay in hospital. She has been kept under observation since, and has been seen from time to time. There is no improvement; she has steadily lost weight, the specific gravity of the urine is always high, 1040 to 1050, and acetone was present in the urine on each occasion. The signs of Graves's disease also are more evident, though the heart's action is quieter.

Dr. Murray points out that not only do the two diseases run concurrently, but that also, if it be looked for, glycosuria not producing symptoms will be found

more frequently than hitherto suspected. Also, as already alluded to here, a considerable number of cases are on record in which Graves's disease has at one time been present and recovered from, and that subsequently true diabetes has developed. That when the two diseases run together the diabetes is always of severe type.

Dr. J. Magee Finny said the subject had been a matter of considerable discussion, as the origin of sugar in the urine had not been satisfactorily settled. He had taken the opportunity of examining several cases of Graves's disease with relation to the question as to whether glycosuria was present or not, and it had been a surprise to him to find that a patient, who had no sugar at first, developed sugar. This was almost at the same time that Dr. Drury's case was in hospital. The subject was a girl, aged eighteen. She had been under his care and daily observation since the autumn. It was not until nearly six weeks after admission that sugar was discovered in the urine, and since then it had been discovered on most of the occasions when observations were made. Sometimes it was in distinct quantities; at other times it could barely be demonstrated; the previous day a quantitative analysis had showed .4 per cent. It was a well-marked case of Graves's disease, with all the phenomena peculiar to the condition. The discovery of the glycosuria made one rather anxious, as the opinion given by most writers was that it was a very bad prognostic, and added greatly to the fatality of a case of Graves's disease. The fear, however, had not been justified in the case, which was proceeding without any very great loss of weight.

Prof. W. H. Thompson said the mere appearance of sugar in the urine was not, after all, saying very much. One had to consider why the sugar appeared, and it was quite acknowledged that disease of the pancreas and total extirpation of the pancreas were very important factors in the production of severe and usually fatal glycosuria. About the connection between it and the thyroid not very much was known physiologically, but from the evidence brought forward he thought there could be no doubt that there must be some orderly connection between disease of the thyroid gland and glycosuria.

Dr. Matson recalled a case of exophthalmic goître of very rapid course. The urine on admission was free of sugar. About the tenth day sugar and diacetic acid were found in the urine. Just before death the sugar disappeared, but the peculiar fruity smell from the patient's breath still persisted.

Dr. Lumsden cited a case which had been under observation for four or five years. The woman had, at first, signs of Graves's disease. She suffered from tachycardia, when the pulse ran up to as much as two hundred. About nine months before death she developed glycosuria and signs of undoubted diabetes. She passed up to two hundred ounces of urine in the twenty-four hours, and had a corresponding thirst. Diacetic acid was also present. She wasted rapidly, and died with well-marked diabetic coma.

Dr. Craig said it had been taught for a quarter of a century that glycosuria might be expected to be found in Graves's disease, and he had made it a rule to have an examination of the urine made for sugar. It occasionally appeared, but in much fewer instances than might be expected from the paper to which they had listened.

Sir John Moore said that the presence of glycosuria in some cases of Graves's disease lent support to the view that the primary lesion in that malady was connected with the thyroid gland (a hyperthyrea), regard being had to the modern doctrine as to the interdependence of the thyroid gland and the pancreas in respect to function.

REMARKS ON THE TREATMENT OF GASTRIC ULCER.

By JAMES CRAIG, M.D., Univ. Dubl., F.R.C.P.I.;
Physician to the Meath Hospital, Dublin.

[Read in the Section of Medicine, January 7, 1910.]

It is not only just but expedient that physicians should recognise and acknowledge the splendid work which their surgical colleagues have accomplished within recent years in the domain of gastric surgery.

So excellent have been their results in certain classes of cases that one is not surprised to find a growing tendency among all sufferers from any variety of gastric trouble to gravitate to the consulting-rooms and hospital wards of the operating surgeons.

Fifteen years ago the physician called the surgeon to his assistance when some calamity, such as perforation, had arisen in connection with the treatment of a gastric case. To-day there are signs that the positions are becoming reversed, and it may be that somewhere in the near future it will rest entirely with the surgeon to decide what cases he will himself treat and what he will relegate to the care of the physician.

Needless to say, the prospect that such a state of affairs may arise is not altogether pleasing to the physician, and, furthermore, I believe, if it does happen, that it will neither tend to the advancement of scientific surgery nor lead to greater confidence on the part of the public. There

is great need that the physician and surgeon should continue to work together, for there is still room for both, and in no section of human ailments is it more necessary that they should work together, and their relative powers and limitations be recognised, than in the case of gastric derangements.

A feeling appears to have gained a hold in the minds of the surgical profession that their medical colleagues have been slow to recognise the possibilities for good that lie within the scope of the knife, and have accordingly been slow in seeking for surgical assistance, whereas the physicians, on the other hand, appear to have become imbued with an idea that surgeons were occasionally reckless in undertaking operations and were not too punctilious in giving up the future care of the patients.

These points of view, and perhaps others, have led, I believe, to a certain amount of estrangement between physicians and surgeons, and the short paper which I now bring before the Academy is written with the best intentions to endeavour to show what the physicians may fairly claim to be able to accomplish, and the extent to which surgical interference may be productive of good in the treatment of cases of gastric ulcer. I trust also that what I have to say may do something to remove any feelings of distrust which the two great branches of the medical profession may entertain towards one another.

My surgical colleague, Mr. William Taylor, has lately mapped out certain lines which he is disposed to follow in regulating his practice in the treatment of gastric ulcer, and I am pleased to say that I am in complete agreement with him in nearly every instance.

The rules which I have formulated for my own guidance are somewhat upon the following lines:—

1. Cases of acute ulcer of the stomach may be con-

fidently expected to get well with a course of sufficient rest combined with suitable dietetic and medicinal treatment.

Should perforation, however, occur, as it sometimes does, even during a course of the above-mentioned treatment, then it is almost needless to say that surgical assistance is imperatively called for.

On the other hand, should hæmorrhage—even several successive hæmorrhages—occur, I am not prepared to advocate operative interference in these acute cases. This is, I think, the only point upon which I am unable to agree with Mr. Taylor, who considers that where a copious hæmorrhage has taken place, and more particularly where there is an immediate recurrence of it, an attempt should be made to secure the bleeding vessel in the floor of the ulcer. His reasoning, however, that a life may be lost which an operation would have saved, is worthy of careful consideration.

2. In all cases of chronic gastric ulcer (of a non-malignant type), unless it is apparent at the time that pyloric obstruction has been already produced by the ulcer, I consider it is only fair to give the patient a chance of recovery in the first instance by a course of treatment similar to that employed in acute cases. I am bound to admit that in the great majority of cases where the symptoms have been in evidence for a year or longer that recurrence of such symptoms takes place within a longer or shorter period, as the case may be, even although a cure appeared to have been effected as the result of the rest combined with dietetic and medicinal treatment. I am convinced, however, that a small proportion of these cases remain permanently well, and for this reason I believe the physician is justified in attempting the cure.

It is in the chronic cases, where symptoms *recur*, that

surgical interference has been followed by the most brilliant results, and it is in such cases that I gladly recognise the good effects that follow the operation of gastro-enterostomy when performed by a skilful surgeon; but it must be admitted that even when we exclude such occasional accidents as the occurrence of the vicious circle or perforation from an intestinal ulcer which has formed at a point opposite the opening of the stomach there is still a margin of cases, whose number is at present unknown, in which all the previous symptoms reappear after operation. Nevertheless, there should be no hesitation in recommending a gastro-enterostomy to be performed in all chronic cases in which there is an undoubted recurrence of symptoms pointing to a chronic ulcer.

The question of the hydrochloric acid content of the gastric secretion has such an important bearing upon the treatment of gastric ulcer, whether the treatment be dietetic and medicinal or whether it be surgical, that I hope you will pardon me for alluding to its significance.

I may say that it is not within the province of this short paper to discuss the diagnostic significance of the absence or presence of free hydrochloric acid in the various derangements of the stomach, although the importance of having a chemical examination made of the stomach contents after a test meal has been given is now fully recognised as affording a distinct help in the diagnosis of functional as well as organic cases. I can only touch at present upon the result of chemical investigations that have been made in recent years in connection with cases of gastric ulcer.

Dr. Willcox, of St. Mary's Hospital, London, published in the *Quarterly Journal of Medicine* for October, 1909, a critical review of the estimation and quantita-

tive significance of hydrochloric acid in the gastric contents, and from this review I extract the following table:—

HCl. may exist as—

I. Free hydrochloric acid—*i.e.*, the acid is not combined with any base, either inorganic or organic.

II. Hydrochloric acid which is combined
  (*a*) With proteins.
  (*b*) With other nitrogenous organic bases.

Active hydrochloric acid

III. Hydrochloric acid which is combined with inorganic bases to form neutral salts—*i.e.*, sodium chloride.

The first two combined he calls "Active hydrochloric acid." They must have been secreted by the stomach, and, as is shown in the table, are equal to the free HCl. + the HCl. combined with proteins and other organic bases.

Many workers, such as Vaughan Harley and Goodbody, in making a quantitative examination of the gastric contents, determine the amount of volatile acid, the amount of free HCl., the amount of HCl. combined with proteins, and also the amount of HCl. combined with minerals.

The point, however, upon which I desire to concentrate your attention is that in the great majority of cases of gastric (and also of duodenal) ulcer the "active hydrochloric acid" is markedly increased, and free hydrochloric acid is present *in excess*. The condition, as you are aware, is spoken of as hyperhydrochloria or hyperchlorhydria.

I cannot here indulge in a recital of the numerous theories that have been put forward to explain the occurrence of a gastric ulcer, but it is now pretty generally accepted that an excess of HCl. in the gastric secretion is an im-

portant factor in its causation. Bolton found that if gastrotoxin is introduced into the wall of the stomach of an animal necrosis and ulcer will result, but not if the HCl. has been previously neutralised by sodium bicarbonate. If we admit, therefore, that there is an excess of HCl. in cases of gastric ulcer; if we further admit that probably the excess of HCl. is an important factor in producing the ulcer; and if it can be also admitted that the presence of the excess of HCl. is an important agent in the production of pain and in the prevention of the healing of the ulcer, then we are in a position to approach the relief of such a condition by methods founded on a sensible basis—that is, we must endeavour to get rid of the excess of hydrochloric acid in the gastric contents. Now let us consider in what way we may hope to accomplish this. First of all, there is the administration of alkalis given with the object of neutralising the excess of acid. At best the relief afforded in this way is only of a temporary nature, even if harm is not done by the use of bicarbonate of sodium, which is so frequently used. Secondly, attention has already been directed to the important fact that HCl. combines in the stomach with proteins given as food, so that the importance of a diet rich in easily assimilated proteins at once becomes evident. Lenhartz took advantage of the fact that HCl. in the gastric secretion combines with protein food, and introduced with great success the treatment of acute gastric ulcer by what is known as the method of "Immediate Feeding," by which much larger quantities of food rich in proteins are given than was previously thought advisable. To the details of this method I shall subsequently refer. Thirdly, the administration of a ferruginous preparation, such as the dried sulphate of iron, may be considered capable of using up some of the excess

of HCl. in the gastric contents by forming an albuminate and a chloride compound.

Investigations have shown, moreover, that there is an excess of HCl. present in cases of chlorosis, and we are familiar with the frequency with which anæmia and gastric ulcer are combined, so that in the administration of an iron preparation in cases of gastric ulcer we may prescribe it with a double object, first as a means of using up the excess of HCl., and, secondly, to relieve the anæmia.

When we come to the question of the treatment of chronic ulcers that have not proved amenable to the methods that meet with success in acute cases, it is surely a tremendous advantage to the physician to be able to rely on the help of his surgical colleagues who will perform a gastro-enterostomy and cure the patients. It is only right that we should endeavour to obtain an answer to the question—How does this operation afford relief and effect the healing of a chronic ulcer?

I have never hesitated to teach that in my opinion a gastro-enterostomy was not a sound or scientific procedure. I have maintained that, where possible, the removal of the ulcer, to be followed afterwards by suitable dietetic and medicinal treatment, would be a more surgical undertaking and more likely to end in permanent benefit to the patient. I recognise, however, that in some cases the removal of the ulcer might be impossible, and that a gastro-enterostomy is at present the operation which surgeons most readily undertake, and although I am not altogether in favour of having a second opening made in the stomach when the pylorus is patent, still I am bound to testify to the excellent results which follow the procedure. Many reasons have been adduced to explain the curative action of a gastro-enterostomy, but it

is now pretty generally admitted that the second opening in some way relieves the hyperhydrochloria.

Willcox, in the criticism before referred to, published the result of the examination of the gastric contents in five recent cases in St. Mary's Hospital before and after gastro-enterostomy for gastric ulcer.

He found that *free* hydrochloric acid was absent in three cases after the operation although present before the gastro-enterostomy was performed, and, further, that in every case the *active* hydrochloric acid was reduced after the operation. Bile and mucin were found in all the cases after the operation, and this has led him and others to assume that these were carried from the duodenum into the stomach through the new opening, and without doubt that the presence of bile, &c., in the stomach had the effect of partly neutralising the HCl. secreted by the stomach. There can be little doubt also that the opening into the intestine favours the escape of the highly acid gastric secretion.

I was under the impression that the view was prevalent among surgeons that the partly-digested food passed directly from the stomach into the small intestine after a gastro-enterostomy had been performed, and thus gave rest to the wall of the stomach and the ulcer it contained. But while one authority states that where the pylorus is patent it is quite easily demonstrated, by means of bismuth in the food and X-rays, that the food continues to escape from the stomach by way of the pylorus another equally eminent observer has just as plainly observed the food passing through the gastro-enterostomy opening directly into the jejunum. If it were beyond question that the food continued to pass through the pylorus, then it removes one of the most serious objections which I had formed against gastro-enterostomies, for it

was apparent that it could not be right to divert the food away from its natural course, where its stimulating effects in the duodenum would be lost on the pancreatic and hepatic secretions.

It is true that in some cases of chronic ulcer, where the symptoms have extended over several years and much inflammatory thickening is found, the active hydrochloric acid may not be much raised or may be only normal in amount, but still it is said (Willcox) that these cases are also cured by a gastro-enterostomy. From this observation one is not unnaturally led to suppose that other factors enter into the cure by operation besides the relief of the hyperhydrochloria.

I now wish to examine the treatment of acute ulceration a little more in detail. Rest, milk diet, relief of constipation, with bismuth and alkaline mixtures, and iron, form the basis of the lines on which most physicians in this country work.

In London rectal feeding has been much more extensively employed than has ever been the case here. In Dublin a milk diet (of from two to three pints per diem) has been attended with excellent results, and is that upon which I have on the whole relied. For many years, however, I have been emphasising the fact that iron is very scantily represented in milk. Stockman in 1891 calculated that five pints of milk would be required to supply the amount of iron required daily by a full-grown adult, and, to quote Hutchison, "for this reason persons who are kept for a long time on a purely milk diet are apt to become anæmic." As previously stated, patients with gastric ulcer are practically always anæmic, and a milk diet makes them more so. Undoubtedly milk is one of the least irritating of foods in causing a flow of gastric secretion—but there is no doubt that in eggs we have

another food which is of the greatest value in the dietary of cases of ulcer of the stomach. The egg albumen readily takes up HCl. and so relieves the hyperacidity. The yolk is of great nutritive value, as it contains protein and a large amount of fat; but furthermore it is comparatively rich in iron which is present as an organic compound, probably with nuclein, and, therefore, as an organic compound it is easily absorbed.

Stockman calculated that seven and a half ordinary-sized (2 oz.) eggs yield sufficient iron to supply the blood of an adult with its daily requirement in this respect.

For these reasons I have been accustomed to add the albumen water and the beaten-up yolk, in soda water, of one or two eggs daily to the milk diet.

Where Professor Lenhartz's treatment has made a departure from recognised methods is in the rapidity with which he increases the amount of nourishment given to the patient by the mouth. I have the strongest objection to be tied down to hard and fast rules which must be slavishly followed in every case irrespective of the special characteristics of the patient or of the disease; but I was agreeably surprised to find how little modification on the whole it required when one comes to put the treatment into practice. The details of the treatment are:—

1. Rest in bed for four weeks, during the first two of which the patient is not even allowed to sit up for any reason whatsoever.

2. An ice bag is kept on to the epigastrium for the first two weeks.

3. The dietary consists of 10 ozs. of milk and one beaten-up egg on the first day, and each successive day $3\frac{1}{2}$ ozs. of milk and one egg beaten up with sugar are added until a total of $1\frac{3}{4}$ pints of milk and 6 or 8 eggs are consumed, and this corresponds with the eighth day. From the

third to the eighth day, according to the disappearance of symptoms, 1 oz. of raw, or nearly raw, minced beef or chicken is given, and increased next day to 2 ozs. if well borne. About the eighth day a little well-boiled rice is given, followed in a few days by softened bread, and then a small amount of bread and butter. The number of eggs is gradually reduced, and one or two of them are given, lightly boiled, as the other food is increased. During the third week more mince and pounded fish are given, and by the end of the fourth week the patient is able to partake of normal diet and to get out of bed. He leaves hospital about two weeks after.

4. As to medicine. For the first ten days 30 grs. of sub-nitrate of bismuth without mucilage are given thrice daily, and towards the end of the first week pills are administered containing dried sulphate of iron and light magnesia. The amount of sulphate of iron may be increased from $7\frac{1}{2}$ grs. to 30 grs. daily.

5. The bowels are not interfered with during the first week, although a natural action may occur, but afterwards an enema, if required, is administered every fourth day.

There is nothing of very much importance in all this detail—nothing that appeals for its enthusiastic and slavish adoption any more than for its scornful rejection. To me it seems that there is included in the method one outstanding principle, the adoption of which, if it can be carried out with safety to the patient, is an advance on previous dietetic methods. This principle is the giving of easily-assimilated protein food in rapidly-increasing amount, so that the excess of free HCl. is taken into organic combination, and by giving a large amount of nutriment the strength of the patient is quickly restored, and the gain in weight is marked.

I have not used the ice bag. Professor Lenhartz is a strong advocate for a supply of good teeth, and instructs his patients to masticate slowly. We all agree in the necessity for teeth that can chew. It is possible that much ill-health and anæmia may arise from continued absorption or the swallowing of septic material in cases of dental caries, but the want of grinders is, I fancy, greatly to blame in the production of gastric ulcer. For the person who is unable to chew falls back upon soft carbohydrate food, which tends to produce excess of HCl. without using it up by forming organic compounds.

I wish to refer very briefly to my own experience in the adoption of Lenhartz's treatment, which I have followed fairly closely in all respects with the exception of the ice bag.

In a case of chronic ulcer with gastrectasis the patient, a labourer, aged twenty-eight years, was put on the treatment on May 5th last, when he weighed 7st. 11lb. He left hospital free of all symptoms on June 22nd, when he weighed 9st. 4lbs. (a gain of 2st. 3lbs.). His symptoms of pain and discomfort returned in August. He again entered hospital on November 4th, when he was transferred to the surgical wards under Mr. Taylor's care, and a gastro-enterostomy was performed and an ulcer found. He is now in excellent health, I am told.

In three cases of acute ulcer in women the results were most satisfactory. One remained in hospital under this treatment from May 25th until June 22nd. She left free from symptoms, was eating ordinary diet, and increased in weight from 7st. 4lbs. to 8st. 7lbs., and is still in good health. The second was placed on the treatment on September 26th, and left hospital on October 31st, having no symptoms, eating all food, and having increased in weight from 7st. 6lbs. to 8st. 7lbs. The third

was placed upon the treatment on November 20th, and she was free from all symptoms, eating all food, and left hospital on Monday, January 3, 1910.

I have tried the immediate feeding on several other cases where the diagnosis was in favour of a neurosis rather than of ulcer, but the results were not satisfactory.

Dr. J. Magee Finny said the raw egg and milk treatment of Lenhartz was very valuable, and he had been satisfied with it.

Dr. Kirkpatrick felt convinced that, apart from any medicinal or dietetic treatment, the most important thing for the patient was absolute rest. He thought it was even more difficult to know when permission might be given for the patient to move than to know when the diet might be increased. The condition of the teeth was a matter of very great importance. If a patient had a septic mouth, there was very little use in treatment until the condition of the mouth was improved. It seemed to him, *à priori*, that rectal feeding, especially at the beginning, ought to give admirable results; but he thought the cases would be few in which it would be tolerated for any length of time, and, even where it was borne satisfactorily, it was really only starvation. Occasionally, for the first two or three days, patients were benefited and pain relieved by withholding all food from the mouth, and giving just enough by the rectum to support life The most difficult of all points in connection with gastric ulcer was to decide what should be done for patients with gastric hæmorrhage. Immediate operation had not been found satisfactory, but if they waited for a second hæmorrhage it might prove fatal.

Dr. Matson spoke of the administration of olive oil, which was supposed to be seized upon by the hydrochloric acid, which it thus neutralised.

Prof. Thompson thought their ideas of the quantity of hydrochloric acid that ought to be normally present in the gastric juice were much too low. Want of teeth and bad mastication would be predisposing causes to fermentation and the production of hyperacidity. It had recently been shown that there was a

normal return of bile and pancreatic juice from the duodenum into the stomach, particularly where there was a large amount of acidity. The giving of foods which promoted the free secretion of acid was a means of causing such return, and connected with the treatment by olive oil. Rest in the recumbent position kept the food more in the part of the stomach where the digestion ought to go on.

Dr. Lumsden spoke in approval of the ice-bag, and agreed as to the importance of the question of the teeth. He had found the results of gastro-enterostomy most admirable.

Dr. Craig replied.

THE PHYSICO-THERAPY OF SCIATICA.

By M. ORB, M.D. University of Erlangen (Germany);
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[Read in the Section of Medicine, February 18, 1910.]

The various methods of physico-therapy have as a common physiological foundation "the using of the natural reactions of the organisms as an end to removing organic disturbances." Although these methods of treatment have been developed separately from medicinal methods, and although there was formerly a great contrast between the representatives of each method of treatment, to-day this antagonism no longer exists. Physico-therapy is taught in many Universities as an important element in our medical armamentarium in full agreement with, and indeed often by, the occupier of the Chair of Medicine. Indeed, both branches of our science "start from the very same point from the pathological condition and clinical experience, and both aim towards the same end—the recovery of our patients." In the treatment of sciatica, which I have selected as my subject, I can see no antagonism between medicinal treatment and physico-therapy, which are rather supplementary one to the other. And although speaking now only about physical methods of treatment I may at once state that I should be sorry to deprive myself of the approved value of the remedies in our materia medica.

Before I go into details I may say that I intend to speak only about the methods which I have found useful in the treatment of genuine sciatica, and that I do not include those cases in which syphilis, diabetes, tumours, &c., produce symptoms of sciatica, and in which the treatment of the cause must be the main object.

I may further abstain from any special classification of sciatica, in so far as it may be neuralgic, neuritic, or perineuritic; for my purpose I think it most practical to discriminate only between acute cases of sciatica and chronic cases.

There are many physical methods of treatment, and they often seem to be opposed to each other. But the underlying principle of all of them is stimulation, and this may be obtained by variations in temperature, by the electric current, by mechanical and chemical means, or by a mixture of all these. If we contemplate our methods from this point of view we can easily explain the same result in spite of the variety of application. Most of them produce a change in the circulation, as well as in the parts of the body immediately touched by the application as in the more remote parts, by reflex action. As a general rule, the more acute the case appears to be the milder must our therapeutic measures be.

Considering first the most actue cases of sciatica we shall give only a treatment that can be easily combined with complete rest in bed. Hot poultices have an old reputation for lessening pain, and the new apparatus—thermophors, electro-thermophors, as well as fangopacks, hot sandbaths, &c.—rest on the same principle. I have not seen much result from cold sprays of chloride of methyl, &c., as recommended by Debove, and prefer the hot application. Hot foot-baths of 110° to 115° F., and about ten minutes' duration, are often very useful, and relieve the pain. In this country I have seen blisters and acupuncture very often applied. In my part of the world they are not often used, as their efficacy, which is certainly beyond doubt, does not generally last long, and it is difficult to repeat them often. I believe dry cupping glasses will have the same effect, and they have this advantage that the patients themselves can use them, and so their application may be frequently repeated. I have seen very

good results in acute cases from wet packs on the hip and leg made with linen or silk bandages, wrung out in water of 60° to 70°, and covered with a double layer of flannel. They get warm very soon, and reduce the pains remarkably well, as I observed in a very severe case just lately. Dr. Friedländer, of Wiesbaden, recommends the renewal of these packs every three hours during the daytime, and leaves them unchanged during the night. I can recommend this remedy very strongly.

Some authors employ massage in the acute cases, but I am quite against using it generally in these cases, especially as the hard pressure often employed by masseurs may considerably increase the pains. But very light friction carefully applied by the doctor himself, avoiding the most painful spots, is sometimes very useful.

Only a few words about electric treatment. I do not apply the electric current very often; if I do, I give the galvanic current in acute and chronic cases. I use two equal-sized electrodes, each 10 square inches in area; the negative I place on the abdomen, the positive on the posterior iliac spine or the sciatic notch, and I move both evenly along the leg without interruption of the current. I seldom give more than 3 M.A., never more than 8 to 10 M.A. I have no personal experience of the action of high-frequency current in cases of sciatica. Some authors, as Kehane, of Vienna, recommend it very much, others express a very reserved opinion or attribute to high-frequency no better success than to the galvanic current. It is difficult to judge, as other methods are usually employed besides electricity, and the pure effects of the latter are seldom tested.

Whilst the application of physical methods of treatment is limited in the acutest cases of sciatica, it has ample scope in the subacute and chronic cases. In these cases massage is used most frequently. It is necessary to employ very

light massage at the beginning of a course of treatment in order to avoid unnecessary and undesirable pains. As improvement takes place, massage may be more energetic. Then tapotement, intermittent pressure, and vibrations over the sciatic notch and the other painful spots are useful. Vibratory massage given by one of the numerous apparatus will be found successful very often. I may mention that generally it is not advisable to give massage for longer than fifteen minutes daily in sciatic cases. Too long an application makes the patients irritable. Once the pain is lessening I recommend gymnastics; and here, too, we must be careful not to overdo them. One must order precisely the special kind of gymnastics and their duration, and must gradually increase them. A little book by Dr. Schreber, entitled "Medical Indoor Gymnastics," published by Williams & Norgate, London, which contains a number of illustrations, has been very useful to me in many instances besides cases of sciatica. As well as gymnastics I order exercises which induce a non-surgical stretching of the sciatic nerve. Zabludowski gives the following advice to that intent:— "The patient should stand at such a distance from the door that he can easily touch the key-hole with his sound foot. He then lifts the affected leg, without bending the knee, as high as possible, pressing the sole of the foot against the door. The height obtained can be marked with a pencil, and the patient must try to get a little higher every day." A similar result may be obtained by passively lifting the stretched leg as high as possible while the patient is lying quietly on his back. I prefer the active stretching, as passive stretching, if not given very cautiously, may make the symptoms worse, whilst the patient surely will not overdo the practice of the active exercise.

The combined effect of massage, gymnastics, and hydrotherapy, as administered in the Vichy and Aix douches, is

very highly recommended, especially in the United Kingdom and France. I appreciate this treatment very much in some cases of chronic rheumatism and gout—I admit, too, its advantage in the treatment of sciatica—but I prefer as a rule to give massage and douches separately. The hands of the masseur slip during the wet massage of the Vichy and Aix douche, and so it is very difficult to obtain an exact and deep effect, and for this reason I generally order the douches entirely separate, either immediately before or after the massage.

The mention of douches leads me to speak of the various methods by which hydrotherapy enables us to attack the disease. I must dwell a little on this subject, as I have found the methods no less useful than other methods and not so well known as they deserve to be. I may mention as very useful the steam douche, the hot water ray, and the Scotch douche. The last has a mechanism enabling us to alternate applications of hot and cold water without any interruption. At the beginning of the treatment I usually give steam douches of 4 to 6 lbs. pressure along the sciatic nerve. When the patient can easily bear this I go on to a hot ray of 110° to 115°, and later on I apply the Scotch douche. I give massage generally before this douche, then put the nerve under the influence of a hot ray of 110° to 115° for one minute, immediately followed by a cold ray for five to ten seconds, and I repeat this alternating douche two or three times. All these douches produce a hyperæmia along the nerve, and any one who has seen the red stripe after the application will understand the great effect. Certainly some cases do not react successfully to the douche ; and here, as in other branches of our science, we must carefully avoid a routine treatment. As the effect is almost the same, I may mention that hot air douches and hot and cold air douches have been often recommended in late years. Formerly

they could be applied only by means of very expensive apparatus, but now some very practical apparatus is constructed, which is applicable wherever electric current is available.

To return to hydropathic treatment, I may mention that plain reclining baths often suit patients very well. They should be 94° at the beginning and of fifteen minutes' duration. During the course of treatment both temperature and duration may be increased—the temperature to 99° or 100°, the duration to half an hour. After them an hour's rest is absolutely necessary. The effect of temperature may be combined with the chemical effect of brine, pine, &c., added to the water, or Nauheim baths, which I do not give for longer than ten to twenty minutes, may be used, the effect of which is chiefly due to the friction of the carbonic acid gas bubbles bursting and regenerating. The value of natural springs depends chiefly on the same principle. Whether the radium emanation found in most of them has a curative power is questionable. If proved to be really efficacious the use of natural springs may become unnecessary, for several companies already sell radium emanation for baths and drinking purposes. However, the time for speaking definitely on this point has not yet come.

The packs I spoke of before may be given with good results in chronic cases, too. Hot sitz baths, which *à priori* might appear useful, are contraindicated, as the overflexing of the thigh during such a bath increases the pain. A few words about the use of Turkish and electric light baths. The effect of both is the same—the former has conducting heat as its efficacious principle in producing a perspiration, the latter radiating heat. As radiating heat produces perspiration by a lower temperature than conducting heat, some patients will find the electric light bath more agreeable, but I do not think that the electric light bath has any specific effect which might influence one to prefer it to other baths which produce per-

spiration. Especially the use of blue light, very often recommended in advertisements of light baths in cases of neuralgia, neuritis, &c., is not warranted. In the treatment of sciatica certainly good results may follow electric light baths of the whole body or of the affected leg; but provided the heart is sound, or at least well compensated, I prefer the use of Turkish baths—we call them Irish baths on the Continent—because the patient can perform gymnastics in them, and these are always easier when the pain is modified by the influence of heat.

Thus we see that we have many methods of physico-therapy to help us in dealing with the disease. I do not wish to finish, however, before trying to anticipate some objections you may have, in so far as you may suppose that these methods are useful in a fully-equipped hospital, but not applicable for the general practitioner. Therefore, I want to point out that most of the forms of treatment described can be easily executed in every private house, and even in the poorest habitation. Some examples may prove it. We find potatoes in the poorest family. Hot mashed potatoes put in woollen cloth are an excellent material for hot poultices, as I often saw whilst practising in a very poor Polish district of Germany. The above-mentioned packs on the hip and the leg require only some yards of old linen, ten to fifteen inches wide, and a suitable piece of flannel as cover can be procured very cheaply. But the douches! If there is a bath-room in the patient's house the doctor may easily adapt an indiarubber pipe to the tap or the shower and apply any kind of douches. In case no bath-room is available, a big watering-pot, of which the rose is removed, enables one to give a douche of any temperature, and taking two—one with hot, one with cold water—we have a very good and cheap substitute for the Scotch douche. As I have said before, all these different forms of treatment depend partly on the effect of varying temperatures, and even admitted that the other chemical and physical stimuli, which

are not available at home, have a good result, we can easily construct substitutes which enable the general practitioner to apply these methods in any, even the poorest, house.

I shall be more than satisfied if this paper influences some of my audience to try these methods in their practice. I am sure they would find great satisfaction and success by doing so.

Dr. Little said he was sure they would all endorse his experience that, among the immense variety of sciatic cases, he had had some that he wished would go to anyone else. He knew a lady who had ruined the reputation of several physicians and surgeons. He had had an unsatisfactory experience of the surgical treatment of nerve-stretching, and his opinion was that in what was called acute cases the one remedy was absolute rest.

Dr. Craig said he was accustomed to deal with neuralgic cases as distinct from the neuritic, and there was no doubt that neuralgia of the sciatic nerve had to be treated in much the same way as neuralgia of any other nerve, and generally yielded to anti-neuralgic treatment. The neuritic cases, however, gave a great deal of trouble. He had been introduced to Corrigan's button many years ago, and had the greatest belief in it, with, of course, some little medical treatment as well. Nerve-stretching was, he believed, associated with considerable danger. A patient of his had had his nerve stretched during his absence, and died subsequently of myelitis. He believed that the hypodermic injection of morphin was one of the few things that gave rest when pain was severe.

Dr. Orb replied.

A CASE OF ACUTE LEUKÆMIA.

By R. TRAVERS SMITH, M.D. Dubl., F.R.C.P.I.,
Visiting Physician to the Richmond, Whitworth and Hardwicke Hospitals;

AND

HENRY C. EARL, M.D. Dubl., F.R.C.P.I.,
Pathologist to the Richmond, Whitworth and Hardwicke Hospitals.

[Read in the Section of Medicine, February 18, 1910.]

Case.—H. R., aged twenty, domestic servant, was sent by her mistress to consult me on October 27, 1909. In general appearance there was nothing noteworthy, except that she looked anæmic. She had auburn hair, fair skin, and was distinctly inclined to be stout. She told me that a fortnight previously she got a bad cold, had generalised pains, and felt shivery. For a few months previously, but more since the cold, she had noticed her nose stopped. On *physical examination* nothing abnormal was found except the nasal obstruction, pallor of the mucous membranes, a few enlarged glands in the right anterior triangle of the neck, and one enlarged gland in the left axilla. Some of her teeth were decayed or septic. There was no fever.

Family history.—Father and mother were living and in good health, as were five sisters and four brothers; none of her immediate family were dead, but some first cousins had died of consumption. Her previous personal history told that she had had pneumonia twice, six and eight years ago, and had had measles and scarlatina in childhood. Not being able to arrive at a definite diagnosis I had advised her to come into the Whitworth Hospital, which she did a week later. On admission her temperature was 102°, pulse 100; her general aspect suggested severe illness; the glands at the right side of neck had become larger, and some could be felt on the left. The spleen was normal in size. The urine contained a little albumen, but no casts.

A blood examination, in the course of a day or two, showed red corpuscles 3,024,000; white corpuscles, 192,000 per c.mm. The hæmoglobin was 55 per cent., lymphocytes 8.1 per cent., polynuclear neutrophiles 3.2 per cent., transitional 88.7. Under these circumstances the diagnosis was made of so-called acute lymphatic leukæmia. From this time on the blood examinations were kindly made by Dr. Earl.

Further progress of the case.—A few days after admission the gums, especially in the region of septic teeth, were noticed very swollen, and a little later they became deeply gangrenous. Separate foci of gangrene arose in the buccal mucous membranes and in the soft palate; these spread rapidly, and coalesced till almost the whole interior of the mouth could be seen as a mass of gangrenous sloughs. Soon after admission the spleen was noted large upon percussion; a few days later it had become palpable; before death it could be felt near the umbilicus. The glands in neck became rapidly larger, so as to produce marked visible swelling. The axillary glands, however, did not become involved, with the exception of one in left axilla previously noted. A moderate diarrhœa persisted through the illness. The tempera ture varied from 101° to 103° till four days before death, when it fell critically. This was attended by a noteworthy diminution in the size of the cervical glands. Even apart from the blood examinations the patient became obviously more and more anæmic, and gradually sank into a semi-comatose state. She died on November 21st, twenty days after her admission into hospital, about five weeks from her first complaint of any definite illness.

Treatment consisted in endeavouring to antisepticise the mouth, the administration of 4,000 units of anti-diphtheritic serum, lest diphtheria bacilli might be among the copious flora that grew from mouth culture. Salol and arsenic were given internally with a view to acting as intestinal antiseptics.

A thorough *post-mortem* was made by Dr. Earl.

On November 9th the blood was examined with the following result:—

Hæmoglobin	-	60 per cent. of normal.
Red cells	-	3,024,000 per c.cm.
White cells	-	192,000 „

There were a few normoblasts, otherwise the red cells were histologically normal. A differential count of the white cells gave—

Small lymphocytes	-	8.1 per cent.
Polynuclear neutrophile cells		3.2 ,,
Transitional cells	-	88.7 ,,

Thus, in a c.cm. of blood there were 6,094 polynuclear neutrophile cells and 15,360 small lymphocytes, the lymphocytes showing an absolute increase and the polynuclear neutrophile cells a slight absolute diminution.

On November 15th the following result was obtained :—

Hæmoglobin	-	30 per cent. of normal.
Red cells	-	1,776,000 per c.cm.
White cells	-	132,000 ,,

Of the white cells there were—

Lymphocytes	- -	13.4 per cent.
Polynuclear neutrophile cells		2.7 ,,
Transitional	- -	83.9 ,,

On November 18th there were—

Hæmoglobin	-	30 per cent.
Red cells	-	1,680,000 per c.cm.
White cells	-	182,000 ,,

A differential count of the white cells gave on that occasion the following result :—

	Per cent.	Per c.cm.
Large lymphocytes - -	1.5	2,720
Small lymphocytes - -	22	40,820
Transitional cells - -	74	134,600
Polynuclear neutrophile cells -	2	3,640
Eosinophile cells - -	.5	910

The number of lymphocytes showed a marked increase during the progress of the case. There were more normoblasts than at the earlier examinations. The transitional cells described here had all the general characters of those in normal blood. They showed red granules with triacid stain, but in well-stained Giemsa or Leishman films fine azurophile granules, and with methylene blue alone very fine basophile granules, were seen.

Post-mortem examination, made thirteen hours after death, the right lung was adherent by numerous firm adhesions to the chest wall. The spleen was very large (it weighed 1,265 grammes)

and very soft. On section it was of a red colour, with areas of darker red and white. The lymphatic glands in the mediastinum were enlarged, but not markedly so. On section they showed caseous areas. A much enlarged gland, which on section was caseous, was removed from the left axilla. The lymphatic glands in the abdomen were not at all enlarged, and there was no enlargement of the lymphoid structures in the intestine. The kidneys were very pale. The liver was very large, but not increased in weight. On section of it a frothy fluid escaped. The organ was spongy in consistence, and portions removed from several parts of it floated in water. The spongy appearance was less marked on the anterior surface, where the organ appeared solid—though it floated in water—than elsewhere. The greater part of the organ had on section much the appearance of a rubber sponge. A Gram-staining micro-organism, which grew anaërobically and produced gas, was isolated from the liver. It had the general characters of *Bacillus phlegmonis emphysematosæ*. The marrow of the shaft of the femur was red and gelatinous in appearance. The other organs were very anæmic, but otherwise normal. The enlarged glands from the mediastinum and axilla were found on histological examination to be tuberculous. The other glands were histologically normal except that about one in four of the cells in the lymphoid tissue was a transitional cell. The marrow of the femur was composed largely of cells resembling transitional cells, and hardly any granules could be made out with triacid stain in films or sections of the marrow. The pulp of the spleen contained many transitional cells. The dark red and white areas noted above were found to be thrombi. The liver showed the cell infiltration commonly seen in chronic lymphatic leukæmia. The cells were partly small lymphocytes and partly transitional cells. The kidney was normal, and showed no cell infiltration.

GLYCOSURIA AND GRAVES'S DISEASE.

By WILLIAM A. WINTER, M.D. Dubl., F.R.C.P.I.;
Physician to Steevens' Hospital, Dublin.

[Read in the Section of Medicine, March 18, 1910.]

The subject of glycosuria and Graves's disease has recently been brought before this Section in a comprehensive manner by Dr. Drury. It is, therefore, not my intention to again go over the ground so recently covered by him, but merely to record a clinical case, in order to emphasise the important subject he brought before us.

M. K., a married woman, was sent into Steevens' Hospital last month by Dr. Atock, and was placed under my care; she is a well-preserved woman, fifty-nine years of age, and on admission weighed 9 stone 7 lbs. She appears to be fairly well nourished, but states that during the last few months she has lost a good deal of flesh. Her family history is good, and her personal history excellent. She states that she has never been ill, unless, indeed, one can class the fact that she has been fourteen times pregnant under that heading. She has eight children alive; one of her daughters has goître, and a brother is also affected with the same disease. The history of her present illness is that six months ago she commenced to suffer from excessive thirst and large appetite. She also noted that she was passing a large quantity of urine and was getting thin. On examination I found that her heart and lungs appeared to be healthy, her temperature was normal, and her pulse 76. I noticed that her eyes were prominent, and when I was examining her thyroid she volunteered the statement that she had had a goître, but it had got well nine years ago. She also told me that at that time she was rather troubled by palpitation, and that the goître used to swell when she was pregnant, and became much smaller again after her confinement, and that it finally got well, without

any treatment, nine years ago. Her recovery does not coincide with this menopause, as she menstruated within the last year. I referred the above statements to Dr. Atock, who tells me that he remembers the woman ten or fifteen years ago as a well-marked case of exophthalmic goître. On admission to hospital the urine showed all the characters found in diabetes, the specific gravity being 1050, the total quantity being from 80 to 120 ounces, and the first examination gave 6.5 per cent. of sugar; there was no albumen and no diacetic acid. At present there are no symptoms of Graves's disease, except her rather prominent eyes, and her diabetic symptoms have improved under treatment, the specific gravity of the urine being 1040 and under, the total quantity being 40 to 60 ounces, and the last estimation of the sugar by my colleague, Dr. Rowlette, gave a percentage of 2.5.

The time that has elapsed since her goître got well is of interest. In the recorded cases the interval between the onset of the diabetes has been quite short in some cases, while in one recorded by Dr. Murray in the *Clinical Journal* of July 28, 1909, the diabetes did not appear for more than ten years after the Graves's disease has been under treatment.

As I did not see this patient when the goître was present I cannot be absolutely certain that she suffered from that disease. Tachycardia does not seem to have been a prominent symptom with her, but we are familiar with the variability of the cardinal symptoms of this disease, and I think that taking her statements, verified as they are by Dr. Atock, I am justified in considering the case to be one of diabetes following on Graves's disease.

Dr. W. G. Smith remarked that other communications on cognate subjects had been before them by Dr. Dawson and Dr. Leeper, and it was advisable not to draw any great distinction between Graves's disease and mental disease. Graves's disease was intimately associated with mental disease, and sometimes passed into true insanity.

Dr. Drury said the case was the fourth recorded in Dublin, in addition to one under his own care. The condition had been considered a fairly rare one, but it would appear that it only required attention to be directed to it to show that it was not so uncommon as had been thought.

Dr. Finny said the question as to whether the thyroid and pancreatic secretion depended on one another was a point of considerable interest. The general view of the association of glycosuria and Graves's disease was that it was a very grave complication.

Dr. Dawson said he did not know that the occurrence of glycosuria in mental disease was comparable with its connection with exophthalmic goître. In his experience the two forms of mental disease in which glycosuria was found were melancholia and chronic dementia, and it had been attributed to part of the general disorder of metabolism that took place in such cases. Glycosuria occasionally followed the administration of thyroid as a drug, and that might possibly be correlated with its occurrence in exophthalmic goître.

Dr. W. G. Smith said they must beware of mistaking a coincidence for a true association. Glycosuria and albuminuria were frequently associated, and in true diabetes it was not rare for albuminuria to develop. There were, undoubtedly, a great number of carbohydrates which they could identify, and there were many varieties of proteins which they could not so easily distinguish. The result was that they had a larger book of scientific evidence in regard to the physiological chemistry of the carbohydrates than of the proteins. Still, the two conditions were liable to go together in clinical practice, and, speaking generally, the appearance of sugar in the urine was, undoubtedly, an example of a disorder of a metabolic function which could easily be upset by a great variety of causes. There were three broad groups of relationship between glycosuria and clinical medicine—first, with the nervous system; second, with the three ductless glands, the thyroid, the pituitary body, and the adrenals; third, with the secretory glands; but there was also room for a small margin of cases referable to the kidney.

NOTES ON A CASE OF ACUTE LEUKÆMIA.

By T. GILLMAN MOORHEAD, M.D., D.P.H., F.R.C.P.I.;
Physician, Royal City of Dublin Hospital.

[Read in the Section of Medicine, May 6, 1910.]

Our knowledge of leukæmia or leucocythæmia has advanced far since Velpeau first, in 1827, recognised the association of changes in the blood and enlarged spleen. To Virchow and Hughes Bennett belong the credit of definitely differentiating the disease, and since then the development of our knowledge has been for the most part gradual, and the landmarks, though important, are few in number. The separation into lymphatic and splenic varieties by Virchow himself; the discovery by Neumann of alterations in the red marrow; Ehrlich's work on the white cells of the blood; Ebstein's discovery of acute lymphatic leukæmia; the more recent description of an acute myelogenous type; and, lastly, the unification of the disease aimed at and suggested by the work of Pappenheim, Grawitz, Dorothy Reed, and others constitute the most important steps that have led us to our present position.

Many problems still remain for solution, not only as regards ætiology and treatment, but also in the relationships which exist between the leukæmias and allied or similar diseases; and in connection with so-called acute leukæmia the most urgent question seems to be the determination whether the blood picture and other pathological changes, which we recognise, are to be regarded as sufficient to justify us in regarding acute leukæmia as a morbid entity, or whether we should look

pon the blood changes at any rate as merely a symptom ommon to a group of acute infections. One of the ways n which this question must be answered is by the more xtended use of careful blood examinations.

Clinically, acute leukæmia presents itself in many ifferent forms. Emerson, in a recent paper in the Johns Hopkins Hospital Bulletin," distinguishes the ollowing varieties:—(1) A common form, suggesting hronic leukæmia, except for its acuteness and severity. n this form there is glandular and usually splenic enargement. (2) An acute infectious type, resembling treptococcic septicæmia, or other well-known acute evers. In some cases, indeed, streptococci have been solated from the marrow, lymph glands, and other parts of the body. (3) A hæmorrhagic form, resembling urpura hæmorrhagica. (4) An acute cachectic group, n which the prominent symptoms are sudden, inexplicable weakness, loss of flesh, and shortness of breath. In none but the first variety are the lymph glands as a rule nlarged, but in all of them stomatitis in varying degrees of severity is an early symptom, and in some, vomiting nd diarrhœa, cutaneous lesions, and pulmonary inflammations are met with. The case which I wish to eport at present, and of which the following are the notes, eems to belong to the second group.

Case.—The patient was a married man, aged twenty-eight, by ccupation a tailor, whom I was asked to see on the afternoon of th April, 1910, and who shortly before my seeing him had passed nto a state of coma. The history that I obtained was briefly s follows:—The patient attended at Mr. Moore's extern dispensary on April 1st, and asked to have some teeth extracted. On examination at the time it was found that his gums were spongy and bleeding, and as he looked ill Mr. Moore admitted him to the hospital. Later the patient stated that he had not

been well during the previous three weeks, and was inclined to attribute his illness to a cold caught when playing football. The symptoms of which he had complained were headache, sleeplessness, pains in the bones, bleeding from the mouth, and loss of appetite.

Previous History.—The patient had always been healthy and active, and had had no previous illness, except diphtheria three and a half years ago, and a troublesome septic sore on the knee three months ago, which had been caused by injury. There was no history of alcoholism or venereal disease. His appetite had never been good, and he had lived much on bread and tea, especially during the last three weeks. He had two children, both healthy; his parents, several brothers and sisters, and his wife were all alive and well.

On examination the patient was found to be sallow and anæmic; the surface of the body was covered with a number of small petechiæ, more especially over the thighs, and on the right buttock there was a considerable ecchymosis; the gums were swollen, bleeding, and foul; the tonsils also were swollen and bleeding, and so was the soft palate; there was one enlarged gland in the right submaxillary region, which was tender to the touch and apparently septic; no other enlarged glands could be detected; the lungs and heart were normal; liver slightly enlarged; spleen not palpable; urine albuminous; temperature, 98.6° F.

On the supposition that the symptoms might be due to scurvy, the patient was placed on anti-scorbutic treatment, and apparently improved, his temperature remaining normal. On April 4th in particular he expressed himself as feeling much better, talked and laughed, and looked well. On the next morning he seemed drowsy, and a little incoherent in his speech, and rapidly passed into the state of coma in which I saw him at 2 p.m. On examination then there was little to add to what I have already related. The patient was deeply comatose; the pupils were equal and reacted to light; the reflexes were dulled; a catheter specimen of urine contained four parts of albumen per 1,000, a good deal of blood, and some blood and granular casts; there was no acetone or diacetic acid. The unusual character of the symptoms suggested the advisableness of making a blood examination, and this immediately established the diagnosis of acute leukæmia.

In addition to ordinary examination of the blood, I withdrew 10 c.cs. from the cephalic vein for bacteriological examination, but cultures of this on broth and agar proved sterile. As showing the tendency to subcutaneous hæmorrhages, it is worth noting that the irritation caused by washing the arm, preparatory to obtaining blood, produced almost immediately a diffuse subcutaneous hæmorrhage. The patient died a few hours after I saw him.

Blood.—A blood count gave the following result :—

Red cells	2,450,000 per c.mm.
White cells	347,500 ,, ,,
Hæmoglobin	60 per cent.
Hæmoglobin index	1.2 (q.p.).
Ratio of white to red cells	1 : 7 (q.p.).

The high hæmoglobin index is worthy of note. It is common in these cases, as M'Crae has pointed out.

Films of blood were stained by Leishman's, Jenner's, Giemsa's and Ehrlich's methods. The large majority of white cells present were mononuclear and of the type described as large lymphocyte, but great variation existed in the appearance of individual cells. In a differential count of 1,000 white cells, the following figures were arrived at :—

	Per cent.	Per c.mm.
Large lymphocytes	90.3	313,792
Small lymphocytes	2.3	7,992
Neutrophile cells	5.1	17,722
Eosinophile cells	1.5	5,212
Hyaline cells	.8	2,780

Four nucleated red cells, all normoblasts, were found in the course of this enumeration. No other abnormal forms of red cells were found. As already stated, great variation existed in the cells classified as large lymphocytes. Some of these were partly disintegrated ; numerous protoplasmic bulgings were seen ; in most the nucleus was spherical and less basophile than the surrounding protoplasm ; in a few indented nuclei were found, and occasionally a distinct nucleolus was evident. In the slides stained by Giemsa's method, the protoplasm of some cells showed faint basophile granulations. Graduations in size from the ordinary small lymphocyte up to the prevailing type of cell,

which was from twice to three times the size of a red cell, were seen. The cells classed as neutrophiles were for the most part devoid of granules, and the name was applied on account of the characteristic nucleus. During the actual count of 1,000 cells no myelocytes were seen, but in more extensive searches a few undoubted eosinophile myelocytes were found. When the list showing the actual number of each form of white cells present per c.mm. blood is studied, an interesting fact is revealed, and that is that both neutrophile and eosinophile cells are present in much greater absolute number than normal. This fact will further be referred to.

A *post-mortem* examination was performed about three hours after death. The lungs were covered with small subpleural hæmorrhages, but were otherwise microscopically and macroscopically normal. Similar hæmorrhages were present on the surface of the heart; the cardiac valves and the aorta were normal; microscopically, cloudy swelling of the myocardium; no definite thymic tissue was found, and there was no enlargement of the thoracic lymph glands. The liver was enlarged, weighing 1,983 grammes; there was considerable vascular engorgement, some cloudy swelling of the liver cells, and slight infiltration of the connective tissue of the portal systems with large mononuclear cells. The kidneys were big, and much engorged; the capsule stripped easily; there was pronounced parenchymatous degeneration; and in places under the capsule small accumulations of round mononuclear cells. The spleen did not extend as far as the costal margin, but was big, weighing 379 grammes. The Malpighian corpuscles were well defined, and not enlarged; the pulp was much more cellular than normal, the predominant cells being large and mononuclear. Cultures made from the spleen proved sterile, and no organisms were found in Gram-stained sections. The peritoneal surface of the bladder and intestines was flecked with numerous small hæmorrhages; the only other abnormality found in the alimentary tract was prominence of the Peyer's patches, with occasional small hæmorrhages into them. Pancreas and adrenals normal; tonsils enlarged, with diffuse hæmorrhages present. Several lymph glands were examined: some of the mesenteric glands were calcareous; others were redder than normal, but not enlarged. The lymph sinuses were distended with large mononuclear cells,

but the lymph nodes could be easily distinguished. A differential count of the cells observed in smears made from the marrow gave the following result, excluding red cells :—

Large lymphocytes	-	95.5 per cent.
Small lymphocytes	-	1.5 ,,
Eosinophiles	-	3.0 ,,

These figures show that the marrow had undergone a very complete lymphadenoid change. The large non-granular cells were apparently identical with those found in the blood, and like those showed numerous small variations.

Sections of all the organs were stained to show the presence of iron, but with the exception of the kidney, which showed traces, none was found.

The acuteness of the case, the paucity of symptoms, the negative results obtained from cultures made from the blood and the spleen, and the large absolute number of eosinophiles present in the blood justify me, I think, in placing it on record.

In thinking over it, numerous points of interest present themselves, and it was at first my intention to deal with some of them in detail, but a review of the literature has shown me that almost every point of importance has already been fully dealt with by Dorothy Reed, Kelly, Whipham, and others, and, in consequence, I will content myself by a brief reference to one or two outstanding features. The origin and nature of the cells that are found in such numbers in the blood is the principal question to be referred to. Ehrlich's view was that two quite distinct varieties of leukæmia existed: the one due to primary disease of the lymph glands; the other to primary disease of the bone-marrow. Pappenheim, on the other hand, ascribes a preponderating importance to the bone-marrow in all cases of leukæmia, and cites as important evidence cases of the kind I have just detailed in which lymphatic leukæmia has occurred with slight or no changes

in the lymph glands, but with important changes in the marrow. That the marrow normally contains some cells of lymphoid nature is now universally admitted, and so there is no longer any *primâ facie* evidence against Pappenheim's view. Ehrlich believes that the marrow changes are the result of invasion by lymphoid cells which have originated in the lymphoid tissues, while Pappenheim holds that the changes are due to active proliferation of the lymphoid cells pre-existing in the marrow. In my case the marrow was almost entirely lymphoid, while in the spleen and lymph glands there was little or no evidence of active proliferation of cells. The spleen pulp and the lymph sinuses were, indeed, packed with large lymphocytic cells, but there was no evidence to prove that these came from the Malpighian corpuscles, or lymph nodes, inasmuch as the cells of those structures were easily distinguishable from the larger cells in other parts of both organs. One can, therefore, adopt Dorothy Reed's conclusion in reference to her own case, that we have in the present case a lymphocytosis arising from proliferation of lymphoid cells in the bone marrow, and death occurring before any other organ in the body showed active involvement. The conclusion arrived at is, in fact, that the changes in the lymph glands and spleen are due to invasion by cells taking origin from the marrow, instead of the reverse being the case as Ehrlich maintains. Further evidence of marrow stimulation is to be found, I believe, in the large absolute number of eosinophiles present in the blood, and in the presence of nucleated red cells before any extreme degree of anæmia had been reached.

What the normal evolution of these cells, known as large lymphocytes, is, seems to be largely a matter of opinion, but the bulk of evidence now tends towards the

view that this cell is the mother cell of all the varieties of blood corpuscles, both erythrocytes and leucocytes. If this be the case, acute lymphatic leukæmia would appear to be a more fundamental disease of the marrow than even so-called myelogenous leukæmia, because the cells attacked would, on the above assumption, constitute the primary marrow cells.

Into the question of the ætiology of leukæmia it is at present fruitless to enter. A careful search was made in the blood films for the rods described by Auer, but without success. I may, perhaps, be pardoned, however, for calling attention to the fact that some years ago I found that extracts from enlarged glands obtained from a case of lymphatic leukæmia produced a striking fall in blood pressure when injected into the veins of rabbits—an effect which was not produced by similar extracts from normal lymph glands. The work was, however, incomplete, and apart from establishing this fact in a single case, was inconclusive.

The last point to which I shall revert is the association of tuberculosis and leukæmia. Sussman some years ago pointed out that tuberculosis and leukæmia are rarely associated. It is, therefore, of interest to note that in the present case there was evidence of old latent tuberculosis in the mesenteric glands, and that in the case recently reported by Smith and Earl active lymphatic tuberculosis was found.

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Numerous other papers were consulted, which it is unnecessary to enumerate, as many of the above give a complete bibliography.

A FURTHER NOTE ON A CASE OF SPLENO-MEDULLARY LEUKÆMIA TREATED BY X-RAYS.

By MAURICE R. J. HAYES, F.R.C.S.I.

Medical Officer in charge of the Electrical and X-ray Department, Mater Misericordiæ Hospital;

Assistant Professor of Materia Medica, University College, Dublin.

[Read in the Section of Medicine, May 6, 1910.]

The patient returned to the Mater Misericordiæ Hospital on March 4, 1910. Since she went to the country, early in December, 1909, she had been quite well until the first week in February, when she noticed that her spleen again began to enlarge, and she had a recurrence of the pains in her legs, back, and the lower part of her abdomen, and she lost all relish for food. Menstruation had been regular and normal.

On examination she looked very anæmic, and she was breathless on the least exertion. Her spleen was hard and immovable; it extended for two inches to the right of the umbilicus and four inches below it, and it filled the entire left flank; it was also palpable beyond the middle line towards the right iliac fossa; it had not enlarged towards the liver in the epigastrium; her liver was not enlarged, nor were the lymphatic glands. She complained of occasional dull headaches; she had no visual disturbances.

On March 4th I gave her two X-ray exposures in the manner already described, one over the lower anterior, and the other over the upper anterior area of the spleen.

On March 9th the condition of the blood was as follows:—

Reds, 2,320,000. (Two megaloblasts seen in a count of 150 leucocytes.)

Hæmoglobin, 65 per cent.

Whites, 229,500. Reds to whites, 10 to 1.

Differential count—	Per cent.
Polynuclears	57.0
Myelocytes (Neutrophiles)	18.5
Myelocytes (Eosinophiles)	3.0
Mast cells	12.0
Broken cells	4.0
Eosinophiles	2.0
Lymphocytes	1.9
Hyaline cells	1.6

On March 11th she was breathless, and her appetite was completely gone. She was very depressed, and she had severe pains on each side of her abdomen and over her sternum. I again irradiated the lowest part of her spleen in her left flank as well as a circular area just above it. With a view to relieving the severe pain in her sternum I also irradiated it, and this pain permanently disappeared within a couple of days.

On March 14th I applied the rays to the upper and anterior part of the spleen in the epigastric region, and on the 21st I gave another exposure beneath the left costal arch.

Despite the treatment she got gradually worse, and she complained of severe pains in her thighs and ribs. She became very breathless, and was vomiting; her feet were œdematous, and her pulse was rapid (116 in the morning and 136 at night). Her temperature varied from 99° to 102° F. There were moist râles over both lungs, and she was worried with a distressing cough which kept her awake at night.

Although there was no improvement in her general condition, her spleen was undoubtedly reduced in size, and when last I saw her it extended only for a hand's breadth below the costal arch. She never had any hæmorrhages. She left hospital on March 30th of this year, when she died on her way to her home in the country.

UNCONTROLLABLE RETCHING—ACID INTOXICATION—OPERATION—RECOVERY.

By ALFRED R. PARSONS, M.D., F.R.C.P.I.;
Physician to the Royal City of Dublin Hospital.

[Read in the Section of Medicine, May 6, 1910.]

On 21st December, 1909, I was asked to see Mrs. A. B., aged fifty-three years, who had been confined to bed since 5th December suffering from weakness, wasting, loss of appetite, dry retching, constipation, with occasional offensive motions, in parts liquid.

Previous History.—She was married in 1886, and has three healthy children, who are now grown up. About 1887 or 1888 she consulted Dr. Little, as she thought her liver was not acting properly. She improved considerably under his treatment, and by following his advice with regard to diet she had comparatively little discomfort, save occasional attacks of "biliousness." Menstruation ceased between six and seven years ago. At that time she was very poorly with hæmorrhage, and was confined to bed for sixteen months. She then got quite strong and well, and bore the strain and anxiety of nursing her husband, who suffered from valvular disease, for some three years. Since his death, eighteen months ago, she has not felt quite well, but has nearly always derived benefit from a change to friends in the country. Last July she sprained her ankle, was consequently unable to take exercise, and got into a low state of health. Early in October she complained of pain in the groins and in the lower part of the back. In the middle of October she left Dublin for three weeks, and her health improved. On her return journey, early in November, she got a slight chill, and felt tired and languid for a day or two, but improved again, and was well until Friday, 3rd December, when she got

another chill. During the following days her appetite failed. She took to bed on 5th December, and retching commenced on the 7th December. At first the retching occurred chiefly at night, but gradually became more frequent, and finally almost incessant. In the fits of retching the patient brought up some mucus, at times bile, but very rarely any food.

She was examined and prescribed for by her brother-in-law, who is a doctor in the west of Ireland, but as the symptoms were increasing rather than abating it was thought necessary to obtain local advice.

On examination I was struck by her anæmic, sallow and generally unhealthy appearance, but then, and subsequently, so far as physical examination went, I could detect nothing to account for her aspect, persistent retching, emaciation and constipation. Her pulse was quiet and her temperature sub-normal. Undoubtedly the right kidney was easily movable, but it slipped back into its normal position with ease.

An examination of the urine, made two days after my first visit, gave the following result: Sp. gr. 1025, acid, haze of albumen, no sugar, acetone present, trace of diacetic acid, no precipitation of phosphates by heat.

On the 29th December, my treatment having failed to check the retching or vomiting, she had become very weak, and looked very bad. Her aspect forcibly reminded me of a nurse whom I had seen for Dr. Moorhead, and subsequently in consultation with him some few years ago. She was about thirty-five years of age, and though she remained on duty, had been looking poorly for several weeks. She then got uncontrollable retching and vomiting, accompanied by great prostration, and urine loaded with acetone and diacetic acid, but containing no sugar. In spite of vigorous treatment she died on the third day after the acetone series of bodies were detected in her urine. The similarity of the cases was strengthened by the presence now in my patient's urine of very large quantities of acetone and diacetic acid, and the absence of a trace of sugar. Nothing could be found in the urine on microscopical examination except a few leucocytes or pus cells. Realising the gravity of my patient's condition, and my inability to account for it, I expressed a

desire for a consultation. Dr. Little, who had seen this lady in 1881 and in 1887, was asked to see her in consultation with me, and at my suggestion her brother-in-law from the west of Ireland also came. In addition to her other symptoms thirst had become distressing, and the treatment I had adopted up to this time had been the administration of alkalies and laxatives by the mouth, and fluid by the bowel containing bicarbonate of soda and glucose.

Dr. Little's opinion of the case may be briefly summed up by one sentence: "I would diagnose malignant disease except for the *dictum*, 'Never diagnose malignant disease except you feel a tumour.'" The vomiting was now frequently bilious. Specimens were examined on three separate occasions. The filtrate was always alkaline to litmus, and, of course, gave no red with dimethyl-amido-azo-benzol. Her brother-in-law, who occupied the next room to the patient, was much impressed by her wretched nights. It was one continual moan. Dr. Little suggested a hypodermic of morphine and atropine and a combination of salicylate, sulphate and bicarbonate of soda, with tincture of calumba and water.

Three days later, although she was passing urine herself, she complained of fulness in the lower part of the abdomen. On examination it was found that her bladder was distended nearly as high as her umbilicus. A catheter was passed, and sixty-one ounces were removed. She had some sleep at night with the hypodermic, but there was no improvement in her signs or symptoms, and she was looking worse. She was now quite unable to pass urine, so the catheter had to be used at regular intervals. The urine still contained large quantities of acetone, and gave a red with ferric chloride, which may have been now in part due to the salicylate of soda.

On 4th January the note is: "No better." She almost lost hope.

On 5th January Dr. Little kindly saw her again. He thought the liver was a little larger than before. The patient said to him: "I cannot live except you can stop this retching."

At our subsequent consultation Dr. Little said: "We do not know what is wrong with this woman. She ought to have her

abdomen opened." I must confess that my own feeling was rather against operation, as I could find no evidence of any lesion likely to be affected by an operation. There was no gastrectasis, no distention of her gall-bladder. I ventured to think there was no calculus in her cystic duct, still less a calculus in her common bile duct, for she was vomiting bile and passing it in her motions, and was not jaundiced, though she was very sallow. There was no intestinal obstruction, and as I have said no tumour present.

I could give no explanation for her vomiting, I must admit. It was certainly not uræmia, and I could suggest no cause for it, except that it was due to acid intoxication, and for this I could only hypothecate some abnormality of metabolism.

The possibility of the vomiting and the other symptoms being neurotic was discussed, but while there was some evidence in favour of this view, such as the temporary improvement which followed the use of a new medicine, we both felt that there was something radically wrong with the patient.

One other point influenced me rather against an operation. It is well recognised that after the administration of any general anæsthetic there may develop uncontrollable vomiting, acid intoxication and death. If this may occur in a patient whose urine is normal before the operation, it seemed much more likely to occur in a woman whose urine already contained large amounts of acetone and diacetic acid.

We decided to ask Dr. Edward Taylor to see the patient with us the following day, and wired to her brother-in-law asking him to come up again.

Dr. Taylor's examination of the abdomen gave a negative result, except that he thought the right rectus was more rigid than the left, and as this observation suggested to him some underlying pathological process he was in favour of a limited exploration. Just at this time I happened to have under my care a patient in the terminal stages of diabetes, and I was enabled to show at our consultation that so far as the acetone series of bodies was concerned, our patient's urine gave an equally or more marked reaction than the diabetic urine.

The question of an anæsthetic was again discussed, and the

possibility of doing it under local anæsthesia considered, but it was resolved that if there was to be an exploration at all it ought to be thorough, and we decided to run the risk of the administration of ether.

Moving the patient to a private hospital was out of the question, and it was decided to tell the patient nothing about an operation till shortly before the time fixed for it.

The patient took the anæsthetic remarkably well, and never gave me a moment of anxiety.

The chief points noticed at the operation may be summarised as follow :—(1) Hardly any bleeding or oozing from the wound. The tissues looked like those at a *post-mortem*. (2) Stomach perfectly normal. (3) Duodenum in first and second stages greatly dilated, without any apparent cause. (4) A cord-like adhesion in upper part of abdomen not pressing on anything was divided. (5) Gall-bladder was dark in colour; was not distended; one calculus could be felt in it. (6) No obstruction of cystic or common bile ducts. (7) Old thickening of hepatic capsule over a small area. (8) Right kidney was movable; left was normal. Both suprarenal capsules apparently normal. (9) No hardening of the pancreas. (10) Appendix normal.

The gall-bladder was incised; two calculi as large as small blackberries ($\frac{5}{16} \times \frac{4}{16} \times \frac{3}{16}$), and nodular on surface, escaped. A glass tube was tied into the gall-bladder to drain it, and the incision closed.

Patient bore the operation very well, and there was no vomiting of any kind whatsoever from the day of the operation.

The bile was collected and examined for acetone and diacetic acid with negative results. The urine contained very large amounts of these substances after the operation, but they gradually got less, and finally disappeared.

An examination of the blood, made the day after the operation, showed R.B.C. = 1,870,000 per c.mm. Smears showed extensive poikilocytosis and some normoblasts, but not more, in Dr. O'Sullivan's opinion, than might be expected with the anæmia present. Six days after the operation she took a little bread, and for the first time for nearly a fortnight passed, without the catheter, a little urine.

On the 14th January the blood was worse :—

Hb.	=	37 per cent.
R.B.C.	=	1,133,000 per c.mm.
W.B.C.	=	1,071 ,,
Colour Index	=	1.5

A capsule of blood was given to Dr. O'Sullivan to determine alkalinity, and he found it was rather higher than normal.

On the 17th January the urine became highly ammoniacal and offensive, with abundant pus in it. She had developed cystitis from the catheterisation.

On the 20th January she was able to pass urine herself, and there was a general improvement in her condition.

On 29th January a blood examination gave the following result :—

Hb.	=	53 per cent.
R.B.C.	=	2,207,500 per c.mm.
W.B.C.	=	3,281
Colour Index	=	1.

Her condition continued to improve. She was able to eat meat and vegetables, and on 19th February her blood showed :—

Hb.	=	65 per cent.
R.B.C.	=	3,161,000 per c.mm.
W.B.C.	=	3,600
Colour Index	=	.92

Since then there has been a marked improvement in her appearance, appetite and strength. Her return to perfect health has been retarded by the cystitis, which, though greatly improved, at times causes much discomfort and vesical irritability. There is still some pus in the urine, but acetone and diacetic acid have long since disappeared.

ABSTRACTS.

SECTION OF MEDICINE.

Friday, October 29, 1909.

The President—Dr. Walter G. Smith, in the Chair.

On some Relations of Physic to Physics, and their bearing upon Therapeutics.

The President gave an Address on above subject. See page 1, *ante*.

Friday, November 26, 1909.

The President in the Chair.

Spleno-medullary Leukæmia treated by X-rays.

Dr. Maurice R. J. Hayes read a paper on this subject. See page 12, *ante*.

Sane Hallucinations.

Dr. Dawson read a patient's autograph account of these. See page 25, *ante*.

Friday, January 7, 1910.

Dr. J. Magee Finny in the Chair.

Glycosuria and Graves's Disease.

Dr. Drury read a paper on this subject. See page 44, *ante*.

Suggestions as to the Treatment of Gastric Ulcer.

Dr. Craig read a paper so entitled. See page 53, *ante*.

Friday, February 18, 1910.

Dr. Little in the Chair.

Dr. Boyd Barrett exhibited the following cases :—

Acquired Imbecility.

J. B., aged five and a half years, a boy, is unable to speak articulately. He can do little more than "grunt." He understands everything said to him, and is somewhat obedient. He did not walk until he was three years old. He is subject to fits of temper, and, as his mother says, "does terrible things," which means that he thinks nothing of throwing china cups over the garden wall. His face is marked by congenital syphilis, the bridge of the nose is sunken, and his mouth is scarred. His mother had several miscarriages. He is the only child that lived. Syphilis is the most common cause of secondary or acquired imbecility. It is also the cause in childhood of that fatal mental degeneration, analogous to general paralysis of the insane in adults, called "juvenile general paralysis." The latter is a progressive and fatal disease. This boy, however, enjoys good health. He is getting stronger every day, and what probably occurred in his case was that, owing to the syphilitic virus, a dystrophy, malformation or anomaly of structure in the brain took place in fœtal life. Dr. G. Still, in his article in "A System of Syphilis," states :—"It may be that syphilis is only an indirect factor in some cases, and that, like any other cause of ill-health in the mother, it interferes with her reproductive power, and so causes imperfect cerebral development in the fœtus without producing in the fœtal tissues or in extra-uterine life any of the specific lesions of syphilis." He goes on to say that parental syphilis may be responsible for congenital idiocy, even when there are no signs of syphilis in the child. A report of twenty-eight cases of idiocy with necropsy (Raviart and Cannæ, *L'Écho méd.*, May, 1909) shows the high *rôle* that syphilis plays in such cases and the variety of changes evident in the brain. Wassermann's reaction succeeded in 47 per cent. of the cases. Amongst the pathological changes were (1) thickening and adherence of the meninges, (2) simple arrest of development, (3) absence or atrophy of such portions of the brain as the frontal

lobes, corpus callosum, &c.; one patient had (4) microcephaly, and another (5) great simplicity of the convolutions; another (6) showed so-called hypertrophy of the brain, and also was a case (7) of myxœdematous idiocy.

Genetous Type of Imbecility, with Single Ptosis.

The patient is a girl, aged ten years. She is quiet and diffident. She has a pale, unhealthy colour, her left eyelid droops, and she shows that type of face and chest which is associated with adenoids. Her expression is vacant. Her speech is jerky and indistinct. The family history on the mother's side was extremely good. The father did not drink to excess. Both parents and the other children in the family were healthy. However, all the near relations on the father's side drank excessively. The child's paternal grandmother was said to have "died of drink." This is of importance. Dr. W. A. Potts, writing recently on the subject (*British Journal of Children's Diseases*, April, 1909), states, in reference to American statistics—"The close investigation of these records shows that often it is not drinking on the part of the parents, but in an earlier generation, that does the mischief." Before discussing the prognosis Dr. Barrett briefly mentioned the stigmata of degeneration present in this case. (1) Physical stigmata—Vacant expression; Gothic palate; winged ears, with deficient helix. (2) Physiological-nocturnal enuresis to age of nine; fæcal incontinence to age of seven; slow in learning to walk and talk. (3) Psychological—Body restless; rhythmic movements of hands; cannot be taught to read, write or sew; loses temper easily; loves dirt and mud. The prognosis must now be considered. This girl, at the age of ten years, can walk and talk. That is about all she can do. She is unable for any serious bodily or mental exertion. She has, unfortunately, a feeble mind in a feeble body. From the grosser defects of childhood she has emerged, but further progress is unlikely. It only remains now to answer two questions. Will she live long? Will she be able to earn her livelihood? The answer to both must be in the negative.

Mongol.

J. O'H., aged five years, is a typical case of this serious condition. With the possible exception of umbilical hernia and congenital

heart disease he has all the chief signs and symptoms of mongolism. He is restless in manner, and chatters unintelligibly but ceaselessly. The complexion is fair. The eyes are oriental, and marked epicanthic folds are evident. He used to dribble and protrude the tongue, but in these respects he has improved somewhat. He has a curious incurvation of the little finger. The head is brachycephalic. Although he is five years old he cannot walk. He was late in sitting up, late in teething, and late in observing. Mongolism is such a serious condition, with such a bad prognosis for life and utility, that it is of the utmost importance to recognise it.

Pseudo-hypertrophic Muscular Paralysis.

Dr. Dempsey showed two cases of boys, brothers, aged eleven and twelve. There was nothing peculiar about the family history, except that an uncle had had some nervous disease. When the elder boy was five or six years of age his mother noticed that he commenced to trip, and had difficulty in going upstairs. In a couple of years he had difficulty in walking, and now, about six years after the onset, he has to be carried. The younger boy was in good health up to a couple of years ago, when he commenced to suffer in the same way. He now showed the combination of atrophy and hypertrophy, which was characteristic of the disease.

Dr. Kirkpatrick, referring to the case of mongolism, said he had been on the watch for the mongol spots in children. The stigma was fairly common in Europe, but he had not met with any, or seen any recorded, in this country.

Dr. Dawson said the case bore out the modern idea of mongolian idiocy. The old idea of syphilis was more or less exploded, and it was now held that it was due to malnutrition of the fœtus in utero, occurring, as it did, when a child was born at the end of a long family. It had been pointed out that the little finger and thumb in such cases were always abnormally short: those symptoms were present in the case. There was also a slightly spotted condition of the child's iris.

Dr. Barrett replied.

The Physico-therapy of Sciatica.

Dr. M. Orb (St. Ann's Hill, Cork) read a paper on this subject. See page 67, *ante*.

Acute Leukæmia.

Drs. Travers Smith and Earl read a paper on the above subject. See page 75, *ante*.

Friday, March 18, 1910.

The President in the Chair.

Bronzing of Skin.

Dr. J. Magee Finny exhibited a case of discoloration of the skin in a boy aged seventeen. It had been first noticed between December and January last. The discoloration was seen very markedly on the neck, trunk, genitals, nipples, and wherever there had been pressure or irritation, such as round the waist. Deep pigmentation had taken place wherever there had been a burn or sore. The knuckles were all very dark; but the face was very little affected. When the boy was admitted to hospital he was filthy in clothes and body. Owing to adverse circumstances he had had to sleep in the stokeholds of steamers at the quays. Dr. Finny therefore thought it might have been the "dirt disease," but on applying a blister the epidermis was quite black when taken away, and was found to contain true pigment. Dark pigment spots were to be seen coming in the patch where the blister had been. The mucous membranes were very little affected, except for a small spot on the lip and two lines on the palate. A number of white marks on the back showed spots which had been torn. The scars, as well as the whole skin, were perfectly soft. The boy was one of a family of seventeen, of whom five were alive. He did not know of what the others had died. His father and mother had both died: one of pneumonia, the other of bronchitis. For some months he had been feeble, probably from want of nourishment. He had no symptoms of vomiting or fainting. He had improved in every way since coming into hospital, though his appetite was not such as they associated with a boy of his age. The kidneys were sound, and the blood-count showed no great loss of red corpuscles; the hæmoglobin was 60 per cent.

Pigmentation in Pernicious Anæmia.

Dr. T. G. Moorhead exhibited a young man who had been admitted to hospital in January last with the typical symptoms of pernicious anæmia.

The President said the condition shown in both cases was an obscure one, about which very little was understood. They did not know the reason for the difference between the white and dark-skinned races of the human family. The only historical landmark which he knew in regard to the diagnosis of the nature of the discoloration was the discovery, in 1846, that the disease now called tinea versicolor was associated with a fungus, and was not pigmentation in the ordinary sense of the word. Pigmentation met with in connection with clinical affections fell into five groups :—(1) In connection with the adrenals ; but he thought that the diagnosis of Addison's disease was, perhaps, hastily arrived at on the mere ground of pigmentation. (2) In connection with the liver ; it had been observed in connection with hypertrophic cirrhosis of the liver. (3) Cases of malaria associated with spleen disease were often markedly pigmented, and in Assam there was a disease called the black disease. (4) There was pigmentation associated with the uterus and ovaries, though there was no pigmentation associated with the male sexual organs. (5) There were cases of dark pigmentation, such as in the epidemic of beer-poisoning some four years ago. Some cases of diabetes were associated with very dark brown pigmentation. Their knowledge of the subject was, however, very incomplete, and the Section was indebted to Dr. Finny and Dr. Moorhead for exhibiting the cases.

Amyotrophic Lateral Sclerosis.

Dr. Matson exhibited a case of this disease. The case was perfectly typical. The symptoms were first noted in October, 1908.

Glycosuria following Graves's Disease.

Dr. Winter read a short account of a case of above. See page 79, *ante*.

On some Early Anæsthetists.

Dr. Kirkpatrick gave an account of the pioneers of anæsthesia, and illustrated a very interesting communication by numerous lantern slides.

Friday, May 6, 1910.

Notes on a Case of Acute Leukæmia.

Dr. T. Gillman Moorhead read a paper on above subject. See page 82, *ante.*

Further Note on a Case of Spleno-Medullary Leukæmia treated by X-rays.

Dr. Maurice R. J. Hayes read an account of the above case. See page 90, *ante.*

Uncontrollable Retching—Acid Intoxication—Operation —Recovery.

Dr. Alfred R. Parsons read a paper so entitled. See page 93, *ante.*

SECTION OF SURGERY.

ON THE TECHNIQUE OF THE OPERATIVE TREATMENT OF FRACTURES OF THE FEMUR.

By R. LANE JOYNT, M.D., F.R.C.S.I.;
Surgeon to the Meath Hospital, Dublin.

[Read in the Section of Surgery, November 19, 1909.]

Fracture of one of the long bones is such a grave accident, and fraught with so serious after results to anyone who has the misfortune to meet with this class of injury, that a consideration of some of the more recent methods of treatment is of prime importance.

The ideal results aimed at in treating any fracture are: Firm union, absence of pain during treatment, complete restoration of function without shortening or deformity.

These ideals have never been obtained in fractures of the femur or humerus when treated by the older methods.

When museum specimens have heretofore been brought forward to demonstrate the overlapping and deformity that may follow a fracture, it has been urged that these were extreme or rare specimens kept for that very reason in museums, and were not to be taken as fair illustrations of the results obtained by the splint and extension method of treatment in the hands of competent surgeons.

Examination of a large number of X-ray photographs proves beyond question that the museum specimens are on the whole a fair representation of what takes place in all fractures of the long bones treated by the older methods.

What do these X-ray photographs show? They show in

every case overlapping of the fragments, imperfect alignment, angular displacement and marked shortening.

In no cases of X-ray photographs which have come under my notice, of fractures of the femur and humerus, and the number is now very considerable, have I failed to find one or all of these imperfections.

The opinions formed from the study of museum specimens, and of X-ray photographs is further strengthened by visual and tactile examinations of the broken bones at the seat of fracture exposed during actual operation.

To anyone who is willing to bring an unbiassed mind to the study of the problem before us, the cumulative effect of this threefold evidence—viz., that supplied by (1) museum specimens; (2) by X-ray photographs; and (3) by actual inspection during operation—must be overwhelming and irresistible. Even unbelievers must be convinced that the older methods are not productive of results sufficiently good to justify the continued adoption of these antique procedures. On the other hand, the results obtained by operation, properly conducted, after due deliberation, by competent surgeons, who have taken the trouble to think out beforehand each and every step necessary for success, are so satisfactory that the surgeon who has once adopted the operative treatment, will never dream of reverting to the older methods save in those cases where from constitutional or other causes any operation would be inadvisable.

Operative treatment, as opposed to that depending solely on splints and extension, consists in cutting down on the fractured ends of the bone and fixing them together in accurate apposition by some positive agent such as wire, screws, or steel plates, which materials may or may not be left buried in the tissues.

It has been objected by those not familiar with modern *technique* in operations that to operate on a simple fracture

onverts it into a compound one—true, but not into an nfected one, for the operator must be as absolutely certain of is asepsis as man can be before undertaking this task.

The horror of a compound fracture, with its attendant uppuration, pain and delayed union, remain as a memory rom pre-aseptic times, when even free drainage was ill under-tood or carried out, but at present, even if wound infection akes place after operative measure, suppuration is never let un riot as formerly, and experience gained from compound nfected fractures, due to the original injury, as well as from ases of operation on simple fractures where infection has ccurred, shows that even then the result following the use f such agents is better than that obtained by other methods.

In this paper I intend to discuss only those methods which my experience has shown me to be the most generally useful. These are, the use of soft iron wire to sew or tie the broken bones together, and the use of flat steel plates having holes in them, which are screwed on to the segments of bone over the seat of fracture, thus joining the broken ends (Mr. Arbuthnot Lane's method).

These wires or plates and screws are intended to remain permanently embedded in the tissues, and experience has shown that only in exceptional cases is it necessary afterwards to remove them.

I will here describe the procedure in the case of a fracture of the femur at its middle third.

The first thing to be done is to have an X-ray examination made. The knowledge thus gained must be checked by exact measurements with the tape. It should be noted how the fragments of the bone lie—and if the fracture is oblique, in what plane the line of fracture is placed. From these observations, coupled with a consideration of the anatomical disposition of the part, it can be decided how the incision should be placed so as to give the best access to the fracture.

Even with the most careful planning beforehand unforeseen circumstances may be found on operation.

When a fracture is comminuted, as shown by the X-rays, the degree of comminution is always greater than would appear from looking at the skiagram. Cracks not yet opened in the bone may not appear in the photo, and on cutting down it may be found that cracks or detached pieces prevent the easy use of a plate, but permit of wiring or *vice versa*, therefore instruments for the application either of wire or steel plates should be ready.

These special instruments for wiring are :—

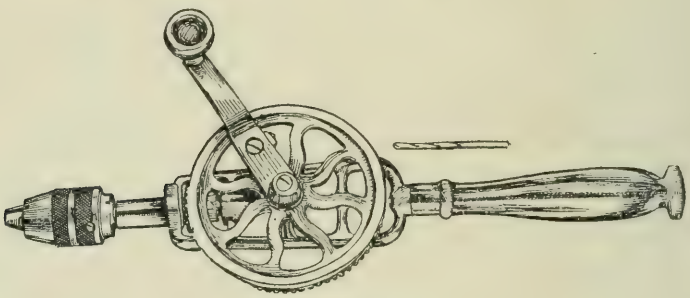

Fig. 1 (Scale $\frac{1}{5}$).

A Wheel Drill (Fig. 1), with twist drill bits for boring holes in the bone. Some surgeons use instead a long drill bit fixed in a handle and used in a similar way to a bradawl. Mr. Lane's drill of this kind is shown in Fig. 2.

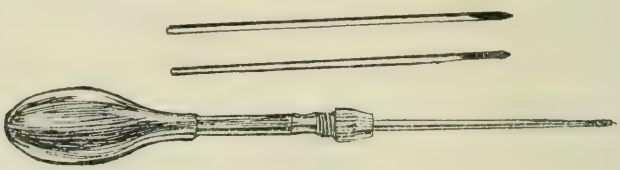

Fig. 2 (Scale $\frac{1}{4}$).

If a wheel drill is used it can be lubricated with sterilised glycerine or sterilised soap solution.

The operator must make himself familiar with the method of inserting the twist drill bits into the chuck or holder of the wheel drill.

The drill bits are very highly tempered, and a few spare ones should be ready in case one breaks. No. 40 Morse gauge is the best size to use.

It should be understood that no drill can bore a hole through two pieces of bone which may move on one another during the drilling, without a great chance of its being jammed or broken. This procedure is on that account inadvisable, and is, moreover, seldom necessary.

The twist drills used are those sold for metal-work—they drill quicker and better than any other form, and cost only a few pence a piece.

The objections to their use are, that if the shank of the twist drill lies against the tissues it will roll up shreds of tissue on itself, unless the assistant is careful to retract the soft

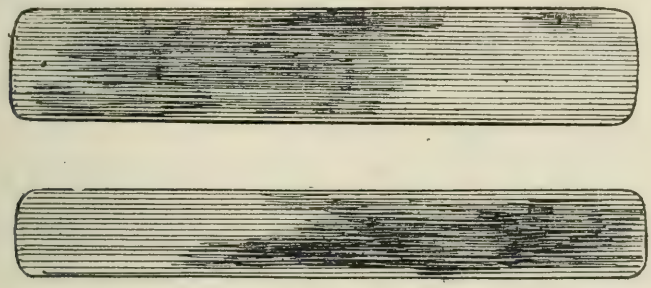

Fig. 3 (Scale $\frac{1}{4}$).

parts, and that surgeons unfamiliar with a wheel drill fail to make the chuck grip the bit securely, and from want of continuous practice have not the steady command over the instrument which ensures its successful use.

Two Flat Spatulas made of copper, silver-plated (Fig. 3). These are flexible, yet rigid enough to retain a bend given them to suit varying conditions met with in the course of the operation.

Their principal use is to retract and protect the sides of the wound from the action of the drill; or by being bent and placed behind a bone, which is to be trimmed with a saw or

drilled, to prevent any chance of either saw or drill injuring the tissue at the side of the bone away from the operator.

They also serve as general wound retractors, and having no gaps in them, the muscles do not bulge through them.

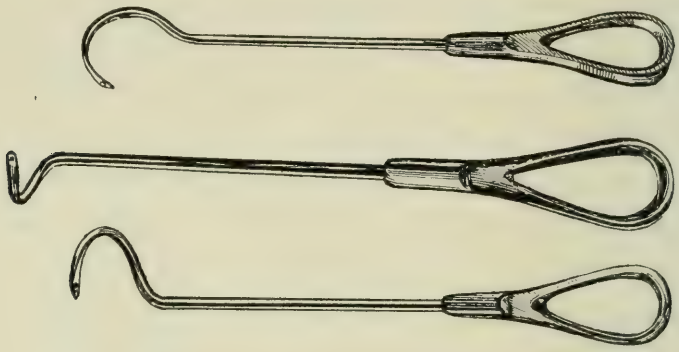

Fig. 4 (Scale $\frac{1}{5}$).

Large Curved Needles (Fig. 4).—These are made like aneurysm needles, only of far greater size and strength. The eye is at the end, the shank is strong, and the handle similar to that of a turn-screw.

They are used for drawing a wire clean around a bone, and in doing this force has to be used both to perforate the strong attachment of muscles, and also to press back the side of the

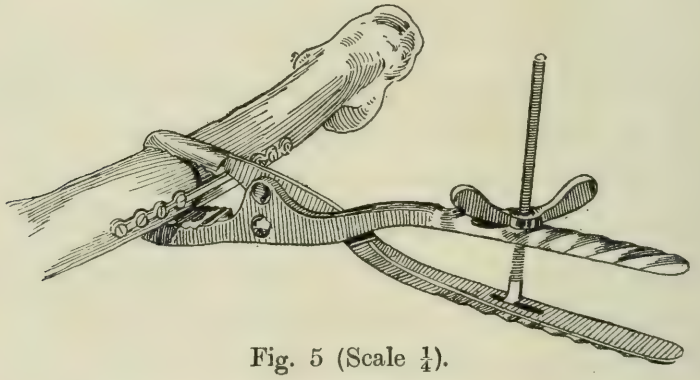

Fig. 5 (Scale $\frac{1}{4}$).

wound in the manœuvre of making the point of the needle appear at the side of the bone opposite to that at which it was entered. The needles may be single or double curved, and are made in varying sizes to suit the bones.

Clamp Forceps (Fig. 5).—This is a device to hold the broken fragments of bone temporarily in position while either drilling or screwing them.

A variety of such forceps is in use.

Peter's clamp forceps resembles an ordinary Lion forceps with two additions. It has a screw attachment to the handles which on being tightened makes the forceps grip without the aid of an assistant, and it has one of its jaws made double to catch the bone over a wider area. This is particularly useful in oblique fractures. It is sold with three blades, which are interchangeable on one another, thus giving the operator a ready choice of the most suitable form.

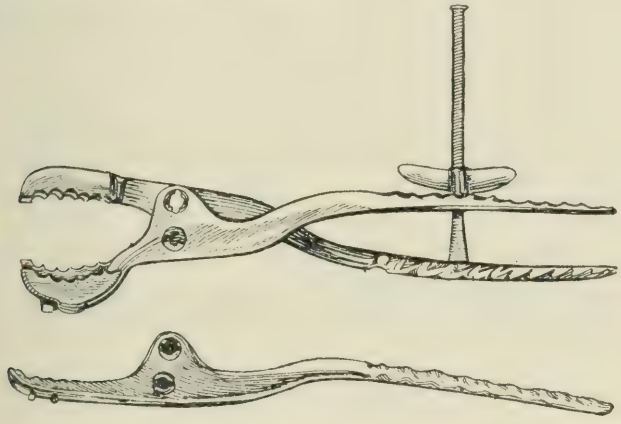

Fig. 6 (Scale $\frac{1}{4}$).

I have had this instrument modified by the addition of a second double-jawed blade, which has proved of great use in holding transversely broken bones in place while a plate was being applied to them.

Fig. 7 (Scale $\frac{1}{3}$).

Levers (Fig. 7) for aiding in the reduction of the fracture. The heaviest lever is a flat polished plate of steel 14 inches in length. Its principal use is to help in the reduction of

transverse fractures where there is much overlapping of the broken ends. It is placed between the broken ends, and using one fragment as a fulcrum, the other end is pressed in a longitudinal direction until the fractured surfaces have come to the correct transverse level with regard to one another. While this manœuvre is being effected with the lever held in one hand, the fingers of the other hand hold the fractured bones to prevent the action of the lever divaricating them. Traction must be made on the limb by the assistants at the same time to relieve the bones as much as possible from undue pressure.

When the fragments are fully extended on one another they can, by the pressure of the fingers, be brought opposite one another, and the lever, which is purposely smooth, is withdrawn.

(Scale $\frac{1}{4}$).

Fig. 8 (Scale $\frac{1}{3}$).

Small Levers (Fig. 8) resemble ordinary surgical elevators or blunt dissectors, but are made somewhat longer and stronger.

They are of use in helping to reduce oblique fractures where it is necessary to place the lever between the fractured surface to move one or other fragment up, down, or sideways to effect proper alignment, or to clear out fragments of bone or tissue which have got between the ends of the bones.

Wire.—Iron wire of No. 20 Imperial Standard is what I have used for many years. It is annealed iron, and is the gauge class of wire that is used by silversmiths and others in holding work together during the process of soldering. It is very soft and tough, can easily be knotted or cut, has no spring in it, and, as

it is very cheap, can be used in lengths which afford ample grip. It is far stronger for its size than any silver or bronze wire, and quite unirritating to the tissues. To prepare it for operation, lengths of two feet should be removed from the hank it is purchased in, and wound without any kinking on to a tumbler. The ends must not be bent or twisted. Several of these two feet coils may be boiled with the other instruments. It is not necessary to remove the black scale from the wire, but it must be free from rust. Kinking or bending wire hardens it and tends to make it break, besides rendering it more difficult to use.

The special instruments for the application of Mr. Lane's steel plates are :—

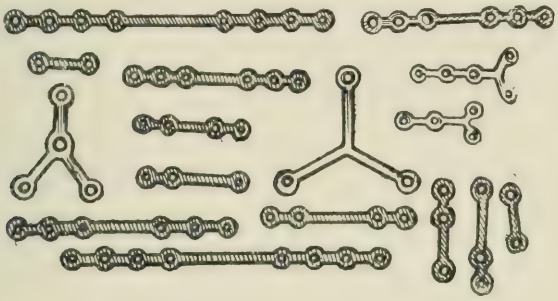

Fig. 9 (Scale $\frac{1}{4}$).

Mr. Lane's Steel Plates (Fig. 9).—*A wheel drill* or other drill as above.

In this case it is of the utmost importance that the twist drill bits be of the correct size for whatever screws the operator may decide on using.

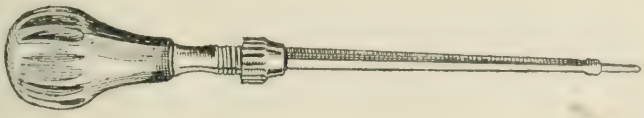

Fig. 10, Reamer (Scale $\frac{1}{3}$).

Reamer (Fig. 10).—This instrument has a tapered square steel point. Its use is to convert the cylindrical hole bored by the drill into a tapered hole to suit the screw. It is used

by rotating it by the handle until the desired diameter of the hole is obtained.

Screws.—Iron screws of $\frac{5}{8}$, $\frac{1}{2}$, or $\frac{5}{8}$ of an inch in length by No. 5 diameter (Nettlefold's) are the best to fit the plates as made by the instrument makers. These screws are the ordinary ones sold for woodwork, and for use on the shaft of an adult femur the $\frac{1}{2}$-inch screw is the best length. The screw is intended to hold in the side of the bone next the steel plate and not to traverse the far side of the bone. This saves the necessity of drilling right through the bone, with the attendant risks of damaging structures beyond it either by the drill or by the point of a projecting screw.

It should be noted that the screws are tapered slightly in their whole length, and to give them the best hold in the bone the reamer ought to make the hole exactly of a similar taper, but slightly smaller in diameter.

Now when the screw is screwed home, its thread or worm just cuts its way into the bone in its whole length, and gradually tightens itself owing to its taper or wedge shape as it goes.

It follows from this that, as bone is a very unyielding substance, that if the hole is too small the screw will jam or break before its head has reached the plate, and if the hole is too large it will not grip. To learn how easily the screws can be broken thus, it is advisable for any surgeon about to undertake this work to practise the procedure either on dry or recent specimens of bones.

Screws are sold specially made for this work, having the thread cut all the way up to the head. These are not necessary, as in wood screws of the size mentioned above the part of the screw known as the barrel—viz., that part without any thread on it below the head—is taken up by the thickness of metal in the steel plate, and in the case of plates used for an adult femur this is $\frac{1}{8}$ to $\frac{3}{16}$ of an inch. It is of vital importance that the surgeon should try the drill and reamer by actual

use on a dry bone to make sure that the reamer leaves the hole the correct size. To prevent the reamer being forced too far into the hole in an endeavour to work quickly, and so rendering the hole useless, I have had the instrument made with a button or guard on it, which, on meeting the steel plate, stops its further entrance (Fig. 10).

Should, unfortunately, the hole be made too big, if a screw of No. 6 diameter is ready it may be used, provided always that the operator has first learnt if the hole in the plate will admit it. But if larger screws than No. 5 diameter are kept ready, they ought not to be mixed up with the others. Their accidental use may lead to one jamming and breaking in a hole not intended for it.

When a screw breaks it always goes at the point where its strong part, the barrel, meets the weak part, the commencement of the thread; as the broken part is flush with or embedded in the bone, it cannot be removed, and must stay there, with, of course, loss of the holding power of the plate.

A reamer may be easily broken, as can also a screw, by another condition which may arise during their use, as follows:—

A hole is bored in the bone through one of the apertures in a Lane's plate already partially fixed by other screws. Now it is found that this last hole is not quite concentric with the aperture in the steel plate. This is due either to boring the hole to one side or to some movement of the limb subsequent to the boring. Now, on applying the reamer, one side of this instrument is trying to cut the steel on one side, and lower down is trying to cut the bone on the other, and the result is breaking off of the point of the reamer. It is obvious that the screw can be broken by getting jammed in the same way.

On account of this risk to the reamer it is well that this

instrument be not too highly tempered, and it emphasises the necessity of keeping the fragments immobilised during the performance of the operation.

Fig. 11, Turn Screw (Scale $\frac{1}{4}$).

Turn Screw (Fig. 11).—This instrument should be carefully ground at its end to fit the slots in the screw heads. American wood screws are far inferior in their finish to English ones, the slots being very badly cut, and it is important to see in buying screws that those of the famous Birmingham house of Guest, Nettlefold, and Chamberlain are obtained.

The best length for a turn screw is about 10 inches. Its shank should be round, so as to permit its rotating in the fingers of the left hand while steadying it.

A long turn screw gives more power than a short one, and permits of the progress of the screw being observed. The reason a long turn screw is more powerful than a short one is that if any angle is formed at the point of contact of the turn screw and the screw head, the hand must, in revolving, traverse part of the circumference of a circle. Now, if this angle is constant, it follows that the longer the shank of the turn screw the longer will be the radius of the circle through which the hand moves, and consequently a crank action is produced, or, in other words, a greater torque is exerted, and this is only limited by the angle at which the turn screw retains its grip in the slot in the screw head. Greater mechanical power, therefore, means less expenditure of energy on the operator's part, and a far finer muscular sense controls his movements. Short, butty, turn screws ought therefore to be avoided.

A forceps to hold the screws while they are being first placed

in the holes is a convenience, owing to the difficulty of working in the depths of a wound, or the awkwardness of working in one or two pairs of gloves. Mr. Lane's type (Fig. 12) resembles a long, delicate artery forceps, only that its blades are bent at an angle to the shanks, and are grooved to hold the screw and yet allow of its rotation.

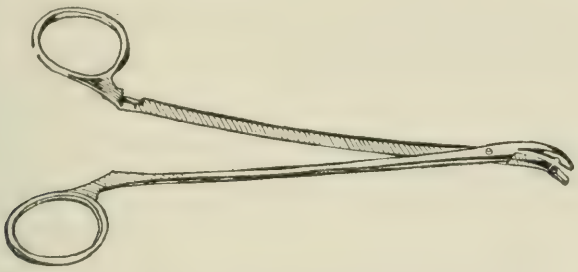

Fig. 12, Screw-holding Forceps (Scale $\frac{1}{3}$).

The clamp forceps, levers, small levers and spatulas described above will be needed also in applying the steel plates.

Besides the foregoing there will be the usual surgical instruments as for any major operation, but not less than 12 clip forceps should be available.

The operator requires four assistants—one to give the anæsthetic, one to help him with the wound, and one to hold the limb below, who must pay special regard to instructions given him, such as to pull, roll out, &c. One is also required to make counter-extension or steady the upper part of the limb.

In the preparation of the patient a preliminary cleansing of the skin should be carried out the preceding day, and after the administration of the anæsthetic the limb must be well cleansed all round, as the operator may have to grip the posterior part of the wound and limb.

The sterilised sheets covering the patient and the table must hang so far over the table that by no chance can the ends of wires touch unsterilised parts, as it may be found advisable to operate at one side of a limb with the patient

lying on his back. By having the patient thus, instead of rolling him on to his side, the surgeon may appreciate better, from observing both limbs in symmetrical positions, what movements may be required to effect reduction. This is especially the case in axial rotation of the distal segment of a limb, or where multiple fractures exist.

The anæsthetic must be so administered that there is as complete muscular relaxation as possible, so that during the actual application of the wires or plates no movement of the patient may take place, and that this quietude may continue until the limb is finally bound up and secured in whatever splint or dressing is decided on.

The assistant who holds the foot or arm must strictly attend to his duties. Any inattention on his part, or an unexpected movement of the patient, especially towards the end of an operation, may break the strongest bone fastenings.

Masks ought unquestionably to be worn. These operations require talking with assistants, and no speaking can occur without risk of wound infection.

Rubber gloves when worn alone cannot avoid getting cut and torn handling sharp bone, but if a pair of fine cotton gloves are put on over the rubber ones, no damage will occur, and, further, they afford a real protection to the hands and fingers which have to do very heavy work, and give a firm grip to drills and instruments.

To avoid an unnecessarily long wound, an incision of 1 to $1\frac{1}{2}$ inches should be made in the selected line over the supposed seat of fracture and down to it. By introducing a finger on to whichever end of the bone is felt in this wound, and ordering extension to be made, it can be best decided which way to continue the incision, so that it may extend an equal way on each side of the centre of the fracture.

In cases where much deformity exists, if this precaution is not observed on extending the limb, it will be found that

portion of the wound inflicted is beyond the area required for access to the fracture. Finally, the cut is extended till the ends of the bone can be both felt and seen.

For the purpose of operation fractures may be considered as either transverse or oblique.

Taking, for example, an oblique fracture of several inches in length, the following procedure may be executed with wire.

The fracture having been reduced by traction, manipulation by the fingers of the operator, and the use of levers or elevators applied to the ends of the bone, and the broken ends brought into perfect apposition, they are firmly held thus by the application of the clamp forceps, the blades of this clamp being themselves then fixed by the screw attachment fitted to its handles.

Three pieces of wire will be required. One piece is to form a loop passed through the bone near the centre of the fracture, and two pieces are to make belts round the bone near the extremities of the break.

The order of their application is as follows :—

First drill the holes required for the central loop.

These holes should be placed so that the loop of wire inserted may have an obliquity corresponding to that of the fracture.

The holes should only go into the medullary canal. They may be drilled with the clamp in position or not, as found convenient. If the clamp is already applied, it will now be necessary to remove it.

Pass a length of wire into one hole from without inwards. Draw its end out of the medullary canal, bend this end slightly, and pass it into the medullary canal of the other fragment and out through the hole in it. Draw it through so as to have an equal length of wire exposed at either end ; leave this wire as yet loose.

Now pass a large curved needle (Fig. 4) close round the bone half an inch from the extremity of the fracture, and

having threaded it with wire, withdraw it. A second belt is passed at the other end of the fracture in a similar way.

These belts are now knotted once, and drawn tight, and as the bone comes into position the central loop is also drawn tight and knotted once. By tightening each knot in turn, accurate apposition can be obtained.

It is not possible, owing to the intervention of tissue between the wire and the bone and the depth of the wound, to tie belts quite tight, but while the cleft in the bone is practically closed, if any looseness exists the pull of the muscles causes some over-riding. The central loop gives a means of overcoming this, for by tying it last it can be pulled on until the cleft is seen to show its serrations corresponding to each other, and any further muscular pull will only draw this loop tighter. In fact, this loop acts as an extensor of the distal portion of the limb, and so it is important to place it as described above, with its direction approximating to the line of the fracture.

When the single knot on any wire is quite tight, the ends of the wire may be twisted twice and cut off, and the ends

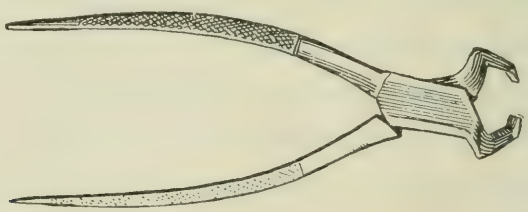

Fig. 13, Dental Forceps for cutting off the wire ends (Scale $\frac{1}{3}$).

turned down. Twisting wire alone without knotting is insufficient, as when strain comes on it the wire unrolls.

The bone being thus united, the wound is closed *secundem artem.*

It can be seen that if the surgeon approaches an oblique fracture so that his incision is in the same plane as the cleft in the bones, the procedure will be very much easier than if he came down on the fracture in any other direction. Where

the cleft is in the sagittal plane, the incision may be made in front, cutting through the rectus and crureus; if, as is more commonly the case, the cleft of the bone lies in the lateral plane of the body, the cut ought to be made towards the outside of the limb. Seeing both sides of the cleft, the serrations of the bone may be made accurately to fit one another, and any marked notch may serve as a guide to locating how to place the central loop of wire.

In the case of a transverse fracture, or one which is only slightly oblique, Mr. Lane's plates may be used as follows:—

Expose and reduce the fracture as already described.

Apply the clamp forceps to the seat of fracture. The forceps made with both jaws double holds the bone quite steady. To permit of either cylindrical or tapered portions of the bone being uniformly grasped, one jaw of the forceps is made to rock sideways on its handle (Fig. 6).

Select a steel plate having eight holes in it. The central portion of the plate should be long enough that no screw-hole will be nearer to the broken ends than half an inch.

Slip the plate under the jaws of the clamp. The gap between the pivot of the clamp and the jaws allows this (see Fig. 5).

See that the plate lies reasonably flat along the bone. Using the plate as a guide to where to drill the screw-holes, proceed to drill, ream, and screw those holes at each end of the steel plate first; as soon as two screws have been inserted into each end of the plate, the clamp may be removed and the remaining screws inserted.

While drilling and reaming the holes attention must be paid to the points mentioned in connection with these instruments. Likewise in inserting the screws no undue force must be used, and once a screw head has gone home the slightest further turning of it will either break it or cause it to shear away the bone projecting between its threads, on which its firm hold depends.

Irrigation of the wound with hot normal saline solution is advisable to get rid of the blood clot due to the original injury, as well as drill dust from the bones.

The wound is now closed in the usual manner. As to whether this may be done in layers with catgut or with a single row of silkworm gut sutures must depend on how the tissues fall together. A wound of the front of the thigh usually comes together with one row of sutures easily, whereas an incision through the outer surface of the thigh may require the muscles and fascia to be sutured in layers.

From a study of these two descriptions, it can be easily seen that the use of the steel plate in the majority of cases effects a quicker and easier method of fixation than the employment of wire, and necessitates less disturbance of the issues, as only one surface of the bone need be exposed.

To secure immobility with a plate, it must be long and have several holes for screws in each end.

The weight of the leg is so great that no piece of steel of a size suitable to be left imbedded in the tissues can be expected to withstand the strain which may be brought on to it if the leg is left unsupported. The same consideration also holds with wire, and this brings us to the matter of subsequent treatment.

Whatever course is followed, it should be borne in mind that a screw put in with proper skill and to the correct degree of tightness will, in a few weeks, become quite loose from the absorption of the bone around it.

It is therefore necessary to guard against any strain coming on the plate or its screws after the first week and until consolidation has taken place. This is why many cases when examined by the X-rays subsequent to the operation, or even after consolidation, show that one or other end of the plate or the screws perhaps seem to have drawn away from the bone, although at the time of operation their position was satisfactory.

If plaster of Paris is used for the splint, a large window must be left over the dressings covering the wound, large enough to permit of the dressings being bodily cut out through it either for removal of the drains of sutures.

The plaster should be strengthened by including in it strips of sheet-iron half an inch broad by one-sixteenth of an inch in thickness.

In the case of the femur the plaster should include the knee-joint and calf of the leg.

The window in the plaster is made by laying on the bandages diagonally and longitudinally around the prominence caused by the wound dressings.

Other methods of splinting and supporting the limb can be carried out with timber or iron splints, but every time they are loosened to remove sutures or change dressings, or to be adjusted, the limb is liable to strain the fastenings unless great care is taken, and this care in after-treatment must be carried out, for reasons mentioned above, for six to eight weeks.

It is generally possible after three or four weeks to remove the plaster splint from the whole leg; and to leave the knee-joint free if another splint is applied later, or to have the splint made removable to permit of massage. While immobilising a joint such as the knee for three to four weeks is not to be recommended, in cases where it can by any possibility be avoided, still here experience shows that a fracture of the shaft of the femur requires about two months for consolidation, and no splint can give support to the limb in the first few weeks unless it embraces the knee-joint.

Details such as are given here may seem superfluous to those who are of a mechanical turn of mind and who, from using their hands in mechanical work other than surgery, are familiar with the qualities of metal and the use of tools. My remarks are intended to help those who are not so trained, although their general surgical skill may be unquestioned.

From questions asked me from time to time, I have concluded that failure to execute these procedures comes more from a lack of knowledge of the details than from ability to grasp the broad outlines. To many non-mechanical persons substances like steel are looked upon as unbreakable, while bone is not given credit for the immense strength it combines with a toughness and elasticity in its fresh state equalled by few other substances. Small wonder, then, that screws may be easily broken in it, or plates and wires smashed by careless work. What takes a long time to describe may be executed by anyone who has taken the trouble to practice on dry bones in a comparatively short time, and I feel confident that the result obtained by the operative measures of treating fractures will well repay the surgeon who practises them, and will relieve many a man from incurring a lifelong disability.

SOME DIFFICULT CASES OF FRACTURE REQUIRING OPERATIVE TREATMENT.

By W. I. de C. WHEELER, F.R.C.S.I.;
Surgeon to Mercer's Hospital, Dublin.

[Read in the Section of Surgery, November 19, 1909.]

There is comparatively no branch of surgery which has undergone so little change since the earliest times as that concerned with the treatment of fractures. Almost as far back as surgical literature can be traced, it has consisted in the use of splints and extension varied from time to time in their mode of application.

Only within the last quarter of a century has any effort been made to treat fractures on some rational, mechanical, and scientific principle, and the attitude of surgeons during this period may be described as one of unrest. Thus we find amongst the revolutions of modern surgery Lucas Championière's advocacy of movement of the fragments while reparative changes are taking place, the general adoption of early movement, and massage when the fracture is in the region of a joint, and the ever-increasing popularity of operative interference. Also, less recognised methods of treatment have sprung up, such as the ambulatory treatment, and the application of Bier's principle of passive congestion, any one of which is sufficient to make the surgeon of fifty years ago turn in his grave.

That unsatisfactory results follow very commonly the treatment of simple fractures by splints and extension there is not a shadow of doubt, and the more the operative treatment is undertaken the more the futility will be realised of dealing with certain cases of simple fractures by the use of splints or extension, however in-

genius the apparatus. Lane says, *inter alia*, "It was evident that the displaced fragments of a broken bone were never or hardly ever restored to their normal position, and that the so-called setting of fractures was a myth." And in another portion of his work speaks in connection with their treatment of the "fossilised reproductions in surgical text-books." More recently Lane has emphasised his former teaching, and states that accurate or anything approaching apposition of displaced fragments in a fracture has been obtained in only very rare cases. The four cases of which I give a short record below are illustrative of the truth of these statements. Many, however, will not be disposed, at present, to accept literally the statement that the treatment of fractures heretofore has been such a hopeless failure as is believed by Lane. They will agree, however, with the more moderate statement of Lambotte, that every fracture, which still remains uncorrected after proper methods of reduction, calls for surgical intervention, and that the correction of the displacements should be controlled by X-ray photographs.

It must be admitted that the advantages claimed for the operative treatment of fractures—viz., the absence of pain, the rapid and perfect union by first intention, and the restoration of the normal skeletal mechanics by absolute apposition of the fragments—are not exaggerated; but there are certain other considerations. It is admitted by all who have had experience in this work that the operation is often anything but easy. There must be a lengthy incision with ample room, the greatest care is required in avoiding important anatomical structures, and the replacing and holding of the fragments in position, or the insertion of the wire, screws and plates is often difficult or impossible. It is just in these difficult

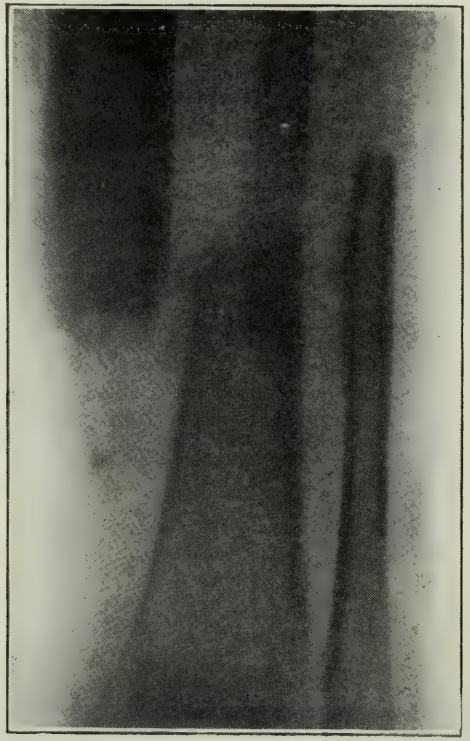

Fig. 1.

Mrs. R. Position of the bones 3½ months after a compound fracture. Before operation.

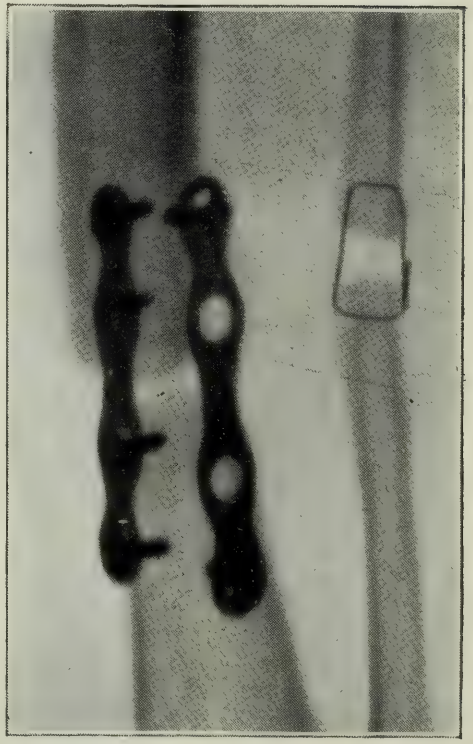

Fig. 2.

Mrs. R. Immediate result of operation for compound fracture of tibia and fibula sustained 3½ months previously.

cases, however, that operative treatment is required; a choice, however, remains in the treatment of superficial and transverse fractures, where accurate position without shortening can occasionally be maintained by the application of a simple splint. The consideration then arises as to whether the increased comfort of the patient during convalescence and the more certain correction of slight displacement justifies, in these simple cases, a proportionably simple operation.

A great deal has been said of the best manner of holding the fragments together when operative interference is decided upon. Wire screws and plates have individual advantages which must be estimated in dealing with each particular case. A glance at the nature of the injury will enable the surgeon to make his choice. I have experienced some of the disadvantages of wire, the difficulty of burying the knot in superficial fractures may necessitate its removal owing to discomfort, or the wire may break and the fragments become displaced unobserved. This occurred in one of my cases. The photograph shows the fibular fragments over-riding to a slight extent in consequence of the failure of the wire to stand the lateral strain (Fig. 5).

If wire is employed, the aluminium bronze is the best. In it strength is combined with fineness and flexibility, and it has the great advantage over other forms, inasmuch as it is possible to tie a knot almost as readily as with silk. If extra strength is required, it is always better to double the wire than to employ coarser material. In all the cases quoted below wire was used in combination with plates fixed with screws. For fractures of the patella and olecranon and all those near joints, pliable plates should be at hand to prevent the tilting of one end as the screw is introduced into the other. Some authorities draw attention to the rarifying osteitis produced by foreign bodies perforating bone, and in order to avoid

drilling recommend that encircling wires should be employed. This rarifying osteitis is probably due to one of two causes, either a slight infection or else the failure to establish complete immobility of the fragments, thus producing friction between the bones and the material used. It is therefore not seen in cases where absolute immobility and asepsis is secured.

All authorities are agreed that if success is to be obtained after operations for fracture, the most rigid asepsis must be ensured. The precautions taken in general surgery may not be sufficient to prevent infection when large quantities of metal are left in a wound. Gloves must always be used in these cases, no antiseptic must on any account be allowed into the wound, and the use of a sharp knife and periosteal elevator should be a substitute for tearing and blunt dissection. Instruments as far as possible should take the place of the fingers in performing the necessary manipulations. Some surgeons recommend drainage for twenty-four hours. It is unnecessary, as is also the use of a tourniquet. If even a small drain is employed, there is an uncomfortable feeling that the little track may not heal owing to accidental infection in spite of all precautions, and will involve the removal of a screw if not the entire plate. In two cases I have employed drainage, and in one after two months time a screw had worked its way to the surface and was removed (Fig. 5). In the other the wound became infected after an operation lasting two hours; the patient was a chronic alcoholic with a fractured femur causing great deformity. It is well to point out, however, that, even if trouble is experienced with the healing of the wound, necessitating subsequent removal of the plates, union of the bones is not prevented. The plates, so long as the screws hold, act as an internal splint until union has taken place, when they are removed to allow healing of the

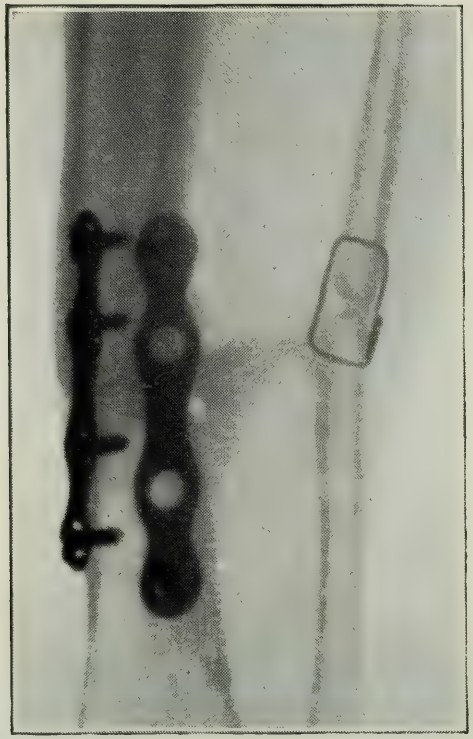

Fig. 3.

Mrs. R. Photograph taken, some months after operation, to show the gap in the fibula filled up and bony

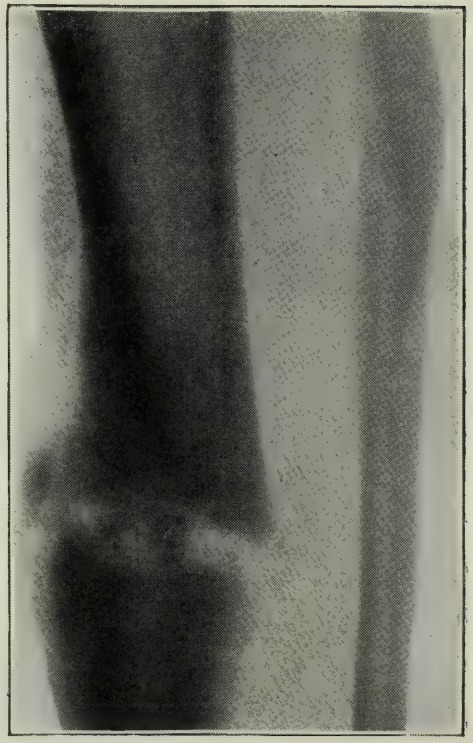

Fig. 4.

Ununited fracture of the tibia and fibula, 11 months' duration. Before operation.

wound. This effect is well seen in the operative treatment of compound fractures.

Case I.—This case came under my care three and a half months after the injury. A compound fracture of both bones of the leg followed a fall down the steps of a church in Strabane. This lady was treated with splints in the usual manner, but after some time it was found that the bones could not be kept in position, and she was sent to a private hospital for operative treatment. An operation was performed in the country, and the ends of the bones were made transverse, and put into position. For three and a half months treatment was persevered in, but at the end of that time no union had taken place, and the bones, when she came under my care, were in the position shown in Fig. 1. Amputation was strongly recommended, as the leg was useless, and she came to Dublin for that purpose. It was decided to give her another chance by cutting down, and, if possible, to free and freshen the ends of the bones a second time, and by extension to overcome as much of the adaptive shortening of the muscles as was possible. This patient was admitted to hospital on March 26th last, but was not operated upon until the 31st, the intervening five days being occupied in preparation of the skin in the area of the intended operation. There was still a small open wound, the result of the original compound fracture and operation; this was disinfected as thoroughly as possible with pure carbolic acid and tincture of iodine. The greatest care was taken to have the technique of the operation as perfect as possible, for it was realised that any failure would most likely necessitate amputation. The aseptic result which followed the operation in the presence of a wound three and a half months old was as gratifying as it was unexpected. The dressings were not removed for a week, the stitches were taken out a week later, and passive movements begun. From this time massage and passive movements were persevered in until her dismissal from hospital on July 21st, nearly five months after her admission, and nine since the date of her accident. Her convalescence was slow, as was to be expected, but beyond swelling of the leg at first after the day's movements she made an uninterrupted recovery. The freshening of the bones on the two occasions alluded to has resulted in about one and three quarter inches shortening. The first photograph after the operation shows a gap between the ends of the fibula

(Fig. 2), and I regretted at the time that in this case a portion of the fibula removed had not been replaced as a graft. The third photograph shows, however, that this gap filled up after a little time, and that a graft was unnecessary (Fig. 3). Recovery has been complete, with perfect use of the injured limb.

Case II.—This patient had an un-united fracture of the tibia and fibula of eleven months' standing, the fracture in the tibia had resulted in the formation of a false joint (Fig. 4). It was hard to understand why, in a transverse fracture of the tibia, a patient without constitutional disease should have developed a false joint. Here, again, one is reminded of the teaching of Arbuthnot Lane, who says that he finds, looking through the text-books, any number of reasons given for non-union of broken bones, the vast majority of which appear to be without foundation. "The surgeon being only too anxious to lay what blame he can upon the patient or his tissues, and inclined to regard as possible any factor other than the obvious inefficiency of his treatment." Again, he says, it may be that in some few cases of simple fracture non-union results from some cause which could not be obviated by proper treatment. In this case, after the patient had been some time under my care, I realised the probable cause of the absence of union. He was of a highly restless and nervous temperament, always believing that he was on the threshold of death, even when convalescent. His splints had to be constantly removed for some imaginary pain, and, finally, suffering one day from a headache, he telegraphed to a brother, who was employed in Sheffield, to come over to Dublin and bring his remains to Galway. The summons was responded to, and the two brothers left for Galway before union was complete. Most probably when he received his injury, eleven months before he was admitted to hospital, he underwent treatment in the same erratic manner. This was one of two cases in which I employed a drain, and whether it was owing to this fact or that the patient was of the restless disposition I have described, one screw loosened and was removed before he left hospital without disturbing the other screws or plates. The leg, which before operation bent under him to about half a right angle on attempting to walk, is now straight, and the bones firmly united. He is still nervous and uncertain as to his ability to bear weight on it, but as time goes on his recovery will be complete. He was admitted to

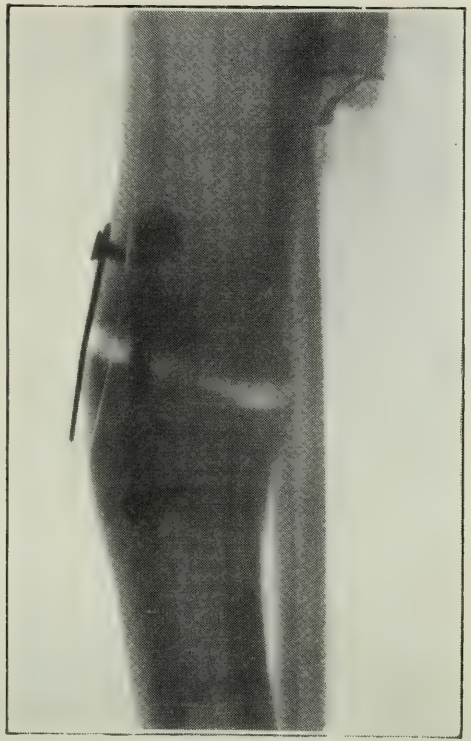

Fig. 5.

Ununited fracture of the tibia and fibula, 11 months' duration. After operation. Note the removal of a screw from the anterior plate. The screw loosened and was removed along

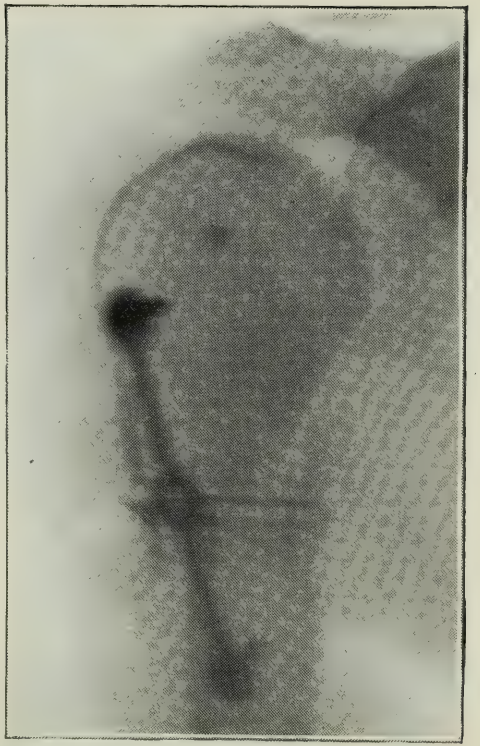

Fig. 6.

Fracture of the neck of the humerus, with marked tilting outwards of the upper fragment. Position maintained by the application of a plate and encircling wire.

hospital on May 10, and operated upon as in Case I, after five days' preparation of the skin. The screw referred to was removed, having worked its way to the surface on July 15. In November he presented himself for examination, with the excellent result stated.

Case III.—This was a case of fracture of the surgical neck of the humerus, which was found impossible to reduce after a fortnight's treatment under the guidance of the fluorescent screen. The upper fragment was tilted outwards in a manner described by Wallis in the *Lancet*, 1906, and the spiculated lower fragment was driven into the muscles under the coracoid process. Jacobson points out that in these cases reduction is often difficult, impossible, or unsatisfactory, and Wallis mentions that, unless operative interference be resorted to, the case will end by excision of the head of the bone. Unfortunately, through an error, only screen X-rays were employed in this case, and no photograph was taken before operation. I was ably assisted by Dr. Flanagan, of Sligo, and the Resident Hospital Staff, and was able, after many endeavours, to get better apposition than that shown in the X-ray (Fig. 6), it is ideal as compared with the position it was possible to obtain during the fortnight's treatment without operation. The patient had no pain or trouble from the time the operation was performed, and now, after three months, she has complete movement and use of her arm. There is no shortening nor other deformity.

Case IV.—This was a child, aged four, who sustained a long oblique fracture of the femur as shown in the X-rays (Fig. 7). The difficulty of approximating the fragments in children, not withstanding the absence of well-developed muscles, is surprising, and the operation is tedious on account of the slenderness of the bone. In some of these fractures of the femur it is remarkable how little extension of the limbs affects the overlapping of the bones—a very strong argument for operative treatment. The X-ray photograph was taken after an attempt to reduce the fracture under an anæsthetic, and to treat it by immobilisation in plaster for a fortnight. The skiagraph taken after operation shows the bone in very good position, held by two plates and screws, and three encircling aluminium bronze wires (Fig. 8). The wire is passed through the centre hole of the plates to prevent it slipping and to still further bind the plates to the bone. This device was also employed in the case of the fractured humerus (Fig. 6). As in

the other cases, the child was free from pain and discomfort from the time the operation was performed. The skin sutures were removed in six days, and from that time the child was untramelled by splints or dressings, with the result that in seven weeks it was able to walk with full movements of the joints No after treatment was required. The two inches' shortening before operation has been completely reduced, and no difference, after careful measurement, can be detected. The child made an absolute recovery with no trace either anatomically or functionally of the very severe injury it received.

In conclusion I wish to acknowledge my indebtedness to the Editor of the "Practitioner" for permission to reproduce the illustrations, and to Dr. Harvey and Dr. Mason for the trouble they took in connection with the photographs.

The President of the Academy said they had had from Mr. Lane Joynt one of the best demonstrations he had ever seen of the application of practical mechanics to surgery; and he thought it a pity that there were not means of teaching something of the subject to medical students. What had struck him most about the instruments shown was their size and business-like capability. The majority of instruments used for bone surgery were small and incapable. He had often speculated as to how the long American spindle turn screw could take out a screw that could not be touched with a short one, and until that night he had never heard anything like a reasonable explanation.

Sir Thomas Myles recalled the fact that he had in 1884 brought forward a suggestion for the operative treatment of fractures. A quarter of a century had passed since then, and thirty years since the introduction of antiseptic surgery, and yet it was only to-day that the operative treatment of fractures was becoming general. They had taken a long time to get out of the routine methods of procedure. Mr. Joynt had shown them that, even when suppuration ensued, if proper provision was made for drainage, the results of operative treatment were infinitely better than those of non-operative treatment no matter how well carried out. It was hardly yet time to dogmatise as to which of the operative methods was the best. Most of them had tried the three methods—the method of wiring, the method of the Lane

Fig. 7.

Long oblique fracture of the femur in a child aged 4. Photograph after 14 days' treatment by plaster of Paris and attempted reduction under an anæsthetic. Considerable deformity.

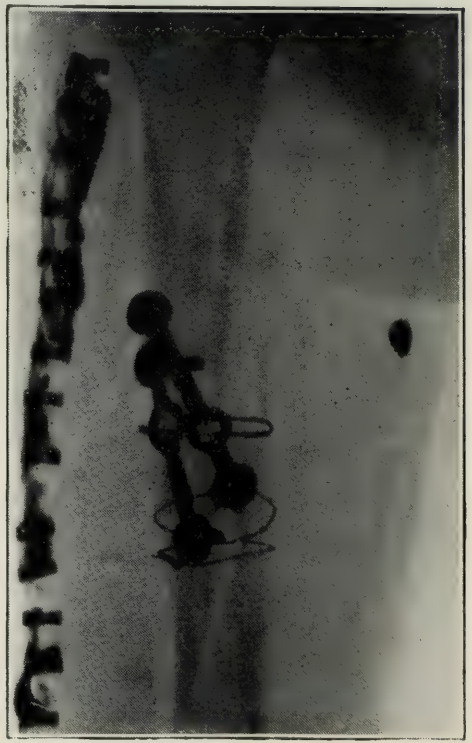

Fig. 8.

Long oblique fracture of the femur in a child aged 4. Result of operation. Note the encircling aluminium bronze wires passing through "screw" holes in the plates. Michel's clips are shown. No splints were used after operation.

plates, and the Parkhill method, which was capable of being used in many cases with less disturbance than the method now being discussed. The particular advantage of any operative method was not shown so much in fractures of the middle of the shaft of the femur as in fractures in the neighbourhood of the joints. He had had five cases of recent fracture under his care. In the case of a man of fine physique who declined to be operated on, and for whom he did everything to make the non-operative treatment a success, the result was a shortening of one and three-quarter inches, and the patient had not recovered the functional use of the knee-joint. In another case of fracture and dislocation of the head of the humerus, the bone was got back into the socket after tremendous difficulty, and anyone could recognise that nothing but bold operative treatment could possibly have given the man a limb of the least use. After two years the patient had a little stiffness in the shoulder. Comparing the results in operative and non-operative treatment, he did not think anyone would hesitate in advising the operative.

Mr. Maunsell cited cases in which he had got good results by wiring, but with great difficulty, whereas when he had used screws and plates in identical cases the operation was not at all a hard one. In a case of compound fracture the plate had come away about five weeks after the operation, but it had not interfered with the result. He did not, however, think that wire had been outdone by screws, operative treatment was not required in every case, as even in the femur he had got good results with plaster, especially in young people, which he did not think could be improved on by operation. He thought it would be unfair to rush into operation before trying other methods. Cases such as those mentioned by Sir Thomas Myles might have to be operated on from the first, but he had had recently two cases of fractured femurs, both of which had gone out after treatment, with no shortening.

Mr. Haughton said those surgeons who viewed the conditions of fracture through X-ray examination became more and more convinced of the necessity for an improved method of treatment. He held personally that it was even more important to take an X-ray picture after the application of splints than before they had been applied. If the position was not satisfactory they could then proceed to operative treatment. He used a small screw-driver on small bones for fear of splitting, but the long screw-driver was more convenient.

Mr. J. Lentaigne said he had been speculating as to how he would treat the case of fracture of the lower end of the femur just above the knee joint in which they might have one of the fragments presenting backwards toward the skin of the popliteal space. He would like to know what incision Mr. Lane Joynt would propose to make for such a fracture, or in separation of the epiphysis in that situation. He remembered two cases of the kind which had been treated by distinguished surgeons, and the results had been lamentable. He himself believed that a good deal could be done by posture. He had treated cases by keeping the leg bent to a right angle, but the position was extremely irksome, and the results, though very much better than former ones, were not to be compared with what they could now get by suturing or plating. If they were to operate at all in a case of fracture, the sooner it was done the better: delay added to the difficulty of the operation and interfered with the beneficial result. He had found the Parkhill clamp difficult to employ: it left several holes in the skin, and there was always greater danger of sepsis. The greatest danger of sepsis was not during the operation, but during the first week or two after the operation.

Mr. Lane Joynt, in reply, said that in approaching the knee for a fracture in the lower third, they would have to go from the outer side, and probably apply a plate. It was easier to work from the outer side than from the inner, and he thought also that they would be able so to avoid the capsule of the joint and the prolongation of the synovial membrane. He would not, however, object to opening the joint, as it could be done without necessarily destroying it. If they were going to place a plate on the bone in the neighbourhood of the joint, it should be, if possible, not in a place exposed to friction. He would not use wire there no matter how tempting it might look. As regards Parkhill's method, he looked upon it as being superseded in some cases; but there were others, notably where they might have a compound fracture of the tibia with excessive suppuration and necrosed bone, in which it would be folly to attempt to suture or screw with Lane's plates, or wire. Wire was not outdone by any means. There were times, as when dealing with fractures in young children, when they should use fine wire. In such cases the shortening would be the same, when the patient was fully grown, as in childhood.

Mr. Wheeler also replied.

SOME POINTS IN THE OPERATIVE TREATMENT OF MALIGNANT DISEASE AFFECTING THE LARGE INTESTINE.

By EDWARD H. TAYLOR, M.D. Univ. Dubl., F.R.C.S.I.;
Professor of Surgery, University of Dublin;
Surgeon to Sir Patrick Dun's Hospital.

[Read in the Section of Surgery, December 17, 1909.]

In order that the operative results in cases of malignant disease affecting the large bowel should show a large percentage of permanent successes the same conditions must be fulfilled as are regarded essential when this disease affects other regions of the body, such as the breast, the tongue, the uterus, &c., viz.:—(1) Recognition of the disease at an early stage of its development, and (2) a sufficiently extensive operative procedure. By this I mean the removal of the primary area of growth and the lymphatic glands which receive the lymph from this part as well as the lymphatic vessels which drain into these glands, and the connective tissue in which they run.

Early diagnosis is the exception rather than the rule. Usually the disease has made considerable local progress before its existence is suspected, and even when symptoms point to its presence valuable time is often lost in treating the patient for supposed chronic constipation.

A reasonable suspicion of malignant disease in the bowel, in the absence of definite signs or symptoms, should be a strong indication for an exploratory operation rather than a policy of waiting until such a degree

of narrowing of the bowel had taken place as to cause obstruction with all its serious consequences.

During such a period of waiting the disease may progress beyond the peritoneal covering of the intestine, with resulting adhesions to adjoining surfaces, and the area of lymphatic infection may become so extensive that no operative measure, no matter how comprehensive, can possibly reach beyond the distal extensions of the disease.

The Extent of the Operative Procedure.—This brings me to an important aspect of my subject, and it is in reference to this that I shall direct the chief remarks which I propose to make.

Hitherto the removal of malignant growths of the large bowel, including the rectum, has been directed chiefly, if not entirely, to the primary focus of the disease, the operator being apparently satisfied in dividing the bowel in a healthy part on the proximal and distal sides of the growth, and at the present day this practice largely prevails, notwithstanding the fact that the operative treatment of malignant disease in other districts of the body, based on accurate pathological knowledge, has assumed a much more comprehensive aspect. Why this obvious imperfection should have persisted in connection with malignant disease of the intestine, or, indeed, I may say, the gastro-intestinal tract, is not quite obvious. The following circumstances may possibly afford an explanation:—

1. It has long been held that malignant disease originating in the tubular glands of the intestine disseminated slowly by way of the lymphatics, and that consequently there was no necessity to carry out wide excisions.

2. Disinclination on the part of operators to extend the

limit of intestinal resection. The frame of mind in which the majority of these cases have been approached has been, in the first place, to determine if a growth exists, secondly, if it is movable, and, thirdly, if its extirpation is possible—in other words, if the mass itself can be removed and the continuity of the tube re-established. The accuracy of my statement is clearly proved by reports appearing from time to time in the various journals of the technique employed in cases of intestinal resection.

Of course, I assume that in these cases the operator finds no marked glandular disease nor visceral metastases.

3. The want of accurate knowledge of the paths of lymphatic drainage leading from the diseased area in the colon has most certainly been the means of retarding progress in this important field of surgery. It is only comparatively recently that serious attention has been bestowed on the subject of the lymphatic vessels and glands in the colon and rectum, the chief investigators being Gerota, Manasse, and Poirier on the Continent; Archibald in America; and in England, Jamieson, of Leeds. Much valuable light has been thrown upon this subject by their researches, and, based upon these, suggestions have been made as to the necessary scope of operations directed towards the removal of malignant disease in the different segments of the colon and its extensions in the neighbouring lymphatic glands.

4. The operative treatment of malignant disease of the colon and rectum is often complicated by the co-existence of intestinal obstruction.

The recognised procedure under these conditions is to relieve the obstruction first by unloading the distended and congested bowel on the proximal side of the constricting growth, so that the subsequent resection may

be carried out under favourable conditions. In the presence of obstruction not only are the bowel contents highly virulent and irritating, but the mucous membrane of the distended part is congested, possibly ulcerated, and its tissues are in a very friable condition. Should resection be performed under such circumstances, a fatal result would be likely to follow from peritonitis, due to leakage occurring in consequence of part of the suture line giving way.

A timely typhlostomy will afford adequate relief of the obstruction in the majority of obstruction cases by establishing bowel drainage. The artificial aperture so created will not occasion any real difficulty when the second stage of the operation is undertaken. Its presence, too, after resection, has a favourable influence on the process of healing at the site of anastomosis owing to the way in which the fœcal current is diverted and serious strain removed from the sutured parts.

In the remarks which I shall make presently as to the scope of operation for cancer of the colon, I shall assume that obstruction does not exist, and shall deal only with that part of the colon which lies to the left of the middle line—viz., from the splenic flexure to the rectum.

I propose to make a few remarks now with reference to the blood supply and the lymphatic drainage of the colon, as they must be thoroughly understood before the question of operative measures can be discussed.

The Blood Supply of the Colon.—The colon is supplied with blood by the superior and inferior mesenteric arteries.

Superior Mesenteric Artery.—The Ileo-colic Artery arises from the superior mesenteric in front of the duodenum. It is directed downwards to the right iliac fossa, and in the vicinity of the ileo-cæcal junction

divides into its terminal branches, viz.:—(*a*) anterior ileo-cæcal, (*b*) posterior ileo-cæcal, (*c*) appendicular, (*d*) ileal, (*e*) colic. It forms an anastomotic arch. Its terminal branch anastomoses with the termination of the parent superior mesenteric vessel.

The colic branch is directed upwards close to the inner border of the ascending colon and anastomoses with the descending branch of the right colic artery.

The right colic artery arises from the superior mesenteric directly in less than 50 per cent. of cases. In about 30 per cent. it has its origin in the ileo-colic artery. It is sometimes represented by a branch of the middle colic and an enlarged branch of the ileo-colic. This vessel is directed outwards to the upper part of the ascending colon, and divides into two branches. Its descending branch anastomoses with the colic branch of the ileo-colic artery, its ascending branch with the right division of the middle colic artery.

The middle colic artery arises from the superior mesenteric just below the body of the pancreas. It proceeds downwards and to the right between the layers of the meso-colon, and about midway between the root of the meso-colon and the bowel divides into two trunks. These diverge, and each again sub-divides into two branches. The four resulting arteries form three distinct arcades. The first of these on the right, opposite the hepatic flexure, is formed in conjunction with the right colic artery. The middle arcade under the right end of the transverse colon is usually small, but the left, which is completed by the left colic artery, is usually large; it lies about two fingers' breadth from the gut, and circumscribes a wide area of the meso-colon, which is devoid of visible blood-vessels. In this arcade it will be seen that the arterial loop is most slender at the

level of the middle with the left third of the transverse colon, and this point may consequently be regarded as marking the line of demarcation between the vascular territories of the middle and left colic arteries.

The middle colic artery supplies the upper part of the ascending colon, the hepatic flexure, and about two-thirds of the transverse colon.

The inferior mesenteric artery arises from the left side of the aorta from one and a half to two inches above its bifurcation. At its origin it is concealed by the duodenum, but is directed from beneath this obliquely downwards over the psoas and left common iliac artery. It now becomes the superior hæmorrhoidal, and descends between the layers of the pelvic meso-colon to the back of the rectum, where it divides into a right and left terminal branch. In its lower part it is accompanied by the inferior mesenteric vein, but this soon leaves it and gradually inclines outwards.

The left spermatic vessels lie about a finger's breadth to its outer edge, and the left ureter descends close to its outer side, coming into very close proximity with it just above the left common iliac artery.

The artery is surrounded by a sympathetic plexus, and its branches are all given off from its outer side; they are the left colic and sigmoid arteries.

THE LYMPHATICS OF THE COLON.

The arrangement of the lymphatic vessels and glands of the small intestine is rendered simple by its single mesentery, but in the case of the colon it is more complicated owing to its peritoneal connections. The glands are most readily described in chains corresponding to the main arterial stems supplying the bowel—viz., the

ileo-colic chain, the middle colic chain, the left colic chain, and the inferior mesenteric chain.

The lymphatic vessels issuing from the colon are directed to glands, some of which are close by, but others are more remotely situated, coming gradually into close relationship with the median or lumbar chain of glands which are scattered along the great vessels in front of the spine.

It is difficult to demarcate the colic glands into definite groups or clusters, as the various members of the series follow upon each other without any definite grouping arrangement as one proceeds from the bowel towards the middle line. It is possible, nevertheless, to subdivide them in a more or less arbitrary fashion. Jamieson and Dobson have sub-divided them, and in connection with each arterial stem they describe (*a*) an epicolic, (*b*) a paracolic, (*c*) an intermediate, and (*d*) a main group.

The epicolic glands are found on the intestinal wall under the peritoneum and in the appendices epiploicæ.

The paracolic glands are arranged close to the mesenteric attachment of the bowel between it and the arterial arcades as well as on the arcades.

The intermediate glands and the glands of the main groups require a separate description for each of the main arterial districts of the colon. Accordingly, I shall briefly review these gland groups in their relationships to the middle colic branch of the superior mesenteric artery, the left colic and sigmoid branches of the inferior mesenteric artery, and the trunk of the artery itself.

In connection with the lymphatic glands which drain any given segment of intestine, the main point of interest is to ascertain what glands may be regarded as primary, by which, of course, is meant those glands which receive afferent vessels directly from the seat of disease, because

the first essential in any operative procedure directed towards the radical treatment of a cancerous growth is the removal of all such primary glands as well as their afferent vessels and the tissues in which these afferents run.

In any part of the colon the epicolic and paracolic glands must be considered as receiving afferents directly from the intestinal wall. Some of the paracolic glands, no doubt, receive efferents from certain of the epicolic glands, and therefore must be regarded as secondary.

The intermediate glands are secondary for the most part as they receive the efferents from the paracolic glands, but in certain districts of the colon lymphatics proceed directly to these intermediate glands; this has been observed most constantly in the region of the hepatic flexure and in the pelvic colon or sigmoid flexure.

The main group glands, so far as their afferents are concerned, are for the most part secondary, or tertiary, in their relationship with the lymphatic drainage of the colon. In some regions, however, certain of these glands must be classed as primary, as they receive vessels directly from the intestine which have eluded the intermediate and paracolic glands on the way. This arrangement has been observed in well-injected specimens in the inferior mesenteric main group.

THE LYMPHATICS OF THE TRANSVERSE COLON.

The lymphatic vessels and glands in connection with this segment of the colon following the usual arrangement are found to accompany closely the blood-vessels, and do not run in the non-vascular intervals.

The epicolic glands lie on the intestinal wall under the peritoneum and in the appendices epiploicæ. They

intercept the majority of the vessels emerging from the intestine. The efferents of these glands and other lymphatics which pass on without interruption reach the paracolic glands or the intermediate group, or even the glands of the main group. This latter arrangement has been observed especially at the hepatic flexure and in the pelvic colon.

The paracolic glands lie along the inner or mesenteric margin of the gut between it and the arterial arcades and on these arcades. As the lymphatics leaving the bowel run with the blood-vessels, those draining the transverse colon associate themselves with the arterial arcades, and consequently stream in one direction or the other between the paracolic glands by which they are intercepted, and do not succeed in getting so far as the intermediate group on the middle colic artery. In this segment of the colon supplied by long arterial arcades it is likely that the paracolic glands at some little distance from the seat of disease may become affected. It is necessary, therefore, to remove the paracolic glands for some little distance on each side of the primary growth.

The intermediate glands surround the stem of the middle colic artery about midway between the origin of the artery and the hepatic flexure—*i.e.*, usually at or close to its bifurcation. They may be somewhat nearer to the origin of the artery and inseparable from the main group. The efferent vessels from those glands pass to the main group.

These intermediate glands receive most of the efferents of the paracolic glands, but rarely any vessels direct from the transverse colon which succeed in passing these without interruption. The frequency with which vessels pass directly to the intermediate glands varies in different regions. They are observed as a rule in the

hepatic flexure, the upper end of the ascending colon, and the right extremity of the transverse colon; also in the pelvic colon. In injected specimens they have not been observed in the central portion of the transverse colon.

The glands of the main group lie on the corresponding artery as it enters the transverse meso-colon. As a rule, they can be differentiated from the glands of the superior mesenteric chain, but sometimes both groups become merged together. These glands of the main group receive the efferents of the intermediate glands, some efferents of the paracolic glands and not uncommonly vessels from the intestine itself.

THE LYMPHATIC VESSELS AND GLANDS OF THE DESCENDING, ILIAC AND PELVIC SEGMENTS OF THE COLON.

The epicolic and paracolic glands have the same topographical relationships to the remaining segments of the colon as have been described in the case of the transverse colon. The epicolic glands are especially numerous in the pelvic colon.

The intermediate group of glands are arranged around the left colic and sigmoid branches of the inferior mesenteric artery.

The intermediate glands related to the left colic artery are situated just above the point of crossing of the inferior mesenteric vein in front of the hilum of the left kidney.

Those related to the lower sigmoid arteries are situated at the base of the pelvic meso-colon. Their relation to the colon is somewhat variable. They may be situated close to it, and are only distinguished then from the paracolic glands by the fact that they lie on the sigmoid arteries, and are capable of being removed without exposing the inferior mesenteric artery. They are situated to the outer side of the inferior mesenteric vein.

Like the corresponding glands in the transverse mesocolon these receive most of the efferents of the paracolic glands, but they also receive some vessels from the bowel which have eluded these glands.

The efferents proceeding from these intermediate glands reach the main group.

The main group of glands related to the left colic artery is divided into two sets. One lies mainly around the horizontal part of the artery near its origin, and is continuous with the inferior mesenteric main group above the origin of the left colic artery. These glands receive some of the efferents of the left colic intermediate group and some of the efferents of the paracolic glands accompanying the branches of the left colic arteries which supply the descending colon.

In none of the injections made by Dr. Jamieson could any lymphatic vessel be traced from the gut directly to this main group.

The second main group of left colic glands lie on the terminal portion of the inferior mesenteric vein in front of the head and at and under the lower border of the body of the pancreas to the left of the superior mesenteric root-glands. This group receives some of the efferents of the left colic intermediate glands, but apparently no direct vessel or any of the efferents from the paracolic glands reach this group. The efferents of this main group communicate with the superior mesenteric glands and with the cœliac and lumbar glands.

The inferior mesenteric main group is related to the stem of the inferior mesenteric artery up to the level of the left colic artery. It joins the left colic group here and becomes continuous with the lumbar glands.

The pelvic colon is closely related to the inferior mesenteric artery at the pelvic brim, and consequently

the lower glands of the main group receive the efferents of the paracolic glands, and frequently direct vessels from the lower part of this colic loop; in this respect they stand in the same relation to the bowel as the intermediate glands in other regions.

The lowest glands of this main group are quite close to the terminal part of the pelvic colon and the commencement of the rectum, and receive the greater number of the direct vessels from the gut, corresponding thus with the paracolic groups elsewhere.

An important fact to note in connection with this inferior mesenteric main group is that all along the entire chain from below upwards efferents pass to the left into the glands of the lumbar group. The inferior mesenteric group, therefore, is not merely continuous with the lumbar glands at its upper part, but along its entire length. It may be regarded, in fact, as a lateral extension of the lumbar group.

The glands of the inferior mesenteric main group receive the efferents of the intermediate group situated on the sigmoid arteries, some efferents of the paracolic glands, and direct vessels from the pelvic colon.

From the diagonally opposite segments of the colon, the hepatic flexure and the pelvic loop, these direct vessels to the main groups are more common than from the intestine between these segments.

PRACTICAL DEDUCTIONS TO BE DERIVED FROM THE NORMAL AND PATHOLOGICAL ANATOMY OF THE LYMPHATICS OF THE COLON.

Although secondary cancer does occur in the glands in cases of cancer of the colon, the invasion is not invariable, and when it does occur it is at a late stage in the progress of the disease.

Advanced primary growths have frequently been found, apparently without any sign of glandular metastasis.

Enlarged glands have been found which failed to reveal evidences of malignant disease on microscopical examination. This is explained by septic absorption from ulcerated surfaces at and above the area of growth.

Recurrence does take place in a large percentage of cases after resection. In 20 cases operated upon by Mickulicz, recurrence took place in 10.

Allowing for recurrence *in situ*, due most likely to persistence of disease existing at the time of the operation, and for metastases of the liver, the great majority of recurrences are due to metastases in lymphatic glands which were not removed.

If glands will become affected at some stage of the disease, and in a certain proportion at an early stage; if it is impossible for the surgeon to determine at the time of operation whether a growth in the colon has given rise to secondary gland infection or not, it is desirable that in addition to the growth those glands should be removed which are likely to have become infected.

All glands, therefore, which can be proved to be related primarily with the seat of the disease should be included in the parts removed, seeing that one primary gland is just as likely to become affected as another, whatever may be their comparative position with regard to the colon (Jamieson and Dobson).

A cancerous growth opposite to a large arcade will require a free removal of the bowel on each side, as it is probable that the paracolic glands at some little distance from the seat of disease may have become affected—*i.e.*, the transverse colon; and to less degree the ascending and descending colon.

The ideal operation necessitates a great extension in

the range of the operations usually practised, and may add something to the immediate risk; but this is amply compensated for by the diminished likelihood of recurrence.

Paradoxical though it may seem, a wide excision of intestine may in some instances be attended with a lower mortality than a purely local excision.

In some districts (splenic flexure) it may be impossible to remove the whole of the lymphatic area.

The question of removal of the secondary glands if the primary glands are obviously diseased now remains.

Reviewing the anatomy of the glands of the colon, it will be seen how impossible it is to remove the secondary glands much though it might be desired.

In some cases, although a local excision of the primary growth may be possible, yet the glandular disease may have extended beyond the primary glands and involved the secondary glands.

Under such conditions it would probably be useless to proceed with the operation except as a palliative measure.

Enlarged glands may be doubtful as to their exact nature. If so, it would be advisable to remove the growth with the primary glands, hoping for the best.

OPERATIVE PROCEDURES.

Splenic Flexure.—Lymphatic Area.—The glands will include those of the epicolic and paracolic groups, and the intermediate glands lying on the branches of the left colic artery. (These were found primarily affected in 4 out of 14 cases.) From the splenic flexure as well as from the left half or two-thirds of the transverse colon there is an additional outlet for the lymphatics between

the layers of the gastro-colic omentum to the glands at the hilum of the spleen.

It is obvious that it will be impossible to remove the lymphatic area in this locality as the splenic glands cannot be efficiently attacked.

The operation will include the excision of the primary growth and that portion of the lymphatic area attached to the branches of the left colic artery.

The Operation.—1. Define the left colic artery outside the inferior mesenteric vein. It is tied here with its vein.

2. Divide the peritoneum to the outer side of the descending colon and splenic flexure and separate the bowel with the growth inwards. The peritoneal tissues and glands are raised.

3. An incision is carried upwards to the transverse colon from the point of ligature of the left colic artery. The level selected for the division of the transverse colon is determined by the area of distribution of the middle colic artery. The left branch of this vessel supplies the transverse colon as far as the junction of its middle and left thirds.

Another incision is carried from the point of ligature of the left colic artery downwards and to the left to the descending colon in its lower part.

The operation is completed by an end-to-end anastomosis, or, if preferred, the two apertures may be closed and a side-to-side anastomosis made by placing the two segments of the colon alongside each other, taking care to ensure isoperistalsis.

The Descending Colon.—In a case of cancer situated in the descending colon the glands which may be affected primarily are the epicolic, the paracolic, and the intermediate glands on the branches of the left colic artery, including those on the first sigmoid artery.

Removal of this lymphatic area may be inadequate, as some vessels from the upper part of the descending colon pass upwards to the splenic glands.

Operation.—1. The left colic artery is exposed in front of the hilum of the kidney to the outer side of the inferior mesenteric artery and ligatured with its companion vein.

2. The peritoneum is divided vertically to the outer side of the descending colon and splenic flexure, and the bowel is then raised together with the peritoneum to its inner side, the sub-peritoneal tissue, blood-vessels, and lymphatics.

3. From the level of ligature of the left colic artery an incision is carried upwards to the transverse colon at the junction of its middle and left thirds.

4. Another peritoneal incision is directed from the ligatured left colic vessels downwards and outwards to the pelvic colon, which is divided in its upper part.

The Pelvic Colon.—This loop of bowel is connected by a mesentery which extends from the left iliac fossa to the front of the third sacral segment, and when empty it usually lies coiled up in the pelvic cavity.

When this loop is affected by cancer the procedure required for its removal will differ according as the growth is situated at its upper or lower part.

It will be most convenient to consider, first, malignant disease in the lower part of the loop.

The lymphatic glands which may be affected primarily from cancer at this level are the epicolic and paracolic close by, and also the glands of the intermediate group and those of the main group along the inferior mesenteric artery.

Examination of injected specimens tends to show that direct vessels proceeding from this part do not enter

glands at a higher level than the middle and upper thirds of the inferior mesenteric artery. This part of the vessel lies well below the origin of the left colic artery.

The apex of the wedge of tissue to be removed will accordingly lie on the inferior mesenteric artery below its left colic branch, and the part removed will include the lower sigmoid arteries, the glands already indicated, the lymphatic vessels, and sub-peritoneal tissue in which they lie.

The upper bowel section will be made in the upper part of the loop. It is not necessary to make it at a high level, seeing that the left colic artery is intact, and the blood supply of the upper part of the pelvic loop assured by the free anastomosis between the left colic and sigmoid branches.

Ligature of the trunk of the inferior mesenteric artery at its origin endangers the blood supply of the upper segment of the bowel: it will then depend entirely on the single anastomosis between the middle and left colic arteries.

Although injection may be made to travel in the cadaver from the middle colic artery into the sigmoid arteries after the inferior mesenteric artery has been tied at its origin, and although it is possible for the circulation of the gut to be maintained in animals after the artery has been tied, it does not follow that such may happen in people advanced in years, perhaps with diseased vessels and a feeble circulation.

The connection between the middle colic and inferior mesenteric arteries may be physiologically inadequate.

The lower line of bowel section is determined partly by the necessity of going well below the growth, but more particularly by a consideration of the blood supply.

The vitality of the lower segment will depend on the

middle and inferior hæmorrhoidal arteries and branches of the sacral arteries.

There is not as yet sufficient evidence to show that these are sufficient to permit of the retention of a portion of the loop at its lower part adjoining the rectum which would render possible a junction of the two segments from within the abdomen.

Operation.—1. The abdomen is opened and the parts are explored as usual. Assuming that the conditions are favourable for resection, the inferior mesenteric vessels are exposed and ligatured just below the origin of the left colic artery.

2. A long incision is made in the peritoneum on the outer side of the pelvic meso-colon and the descending colon up to the splenic flexure. The gut is then freely mobilised by means of dry sponging, and the process of stripping extended inwards towards the middle line. The ureter and spermatic vessels will be exposed and must be carefully guarded.

3. An incision is now carried through the pelvic meso-colon from the point of ligature of the inferior mesenteric vessels to about the middle of the loop, and in doing so the secondary arches on the sigmoid arteries are preserved.

4. Another incision is carried downwards at the inner side of the inferior mesenteric vessels and the promontory of the sacrum and downwards to the inner side of the pelvic meso-colon.

5. The wedge-shaped mass of tissue included between these incisions is then stripped forwards from the upper part of the concavity of the sacrum, and the gut clamped and divided at the middle of the pelvic loop, close to the commencement of the rectum below.

The blood supply of the upper segment is practically

assured by the preservation of the left colic artery; that of the lower end depends on the middle and inferior hæmorrhoidal arteries and is open to doubt.

It may be tested by relaxing the blades of the clamp and observing if the cut edge of the bowel bleeds freely. If the circulation appears satisfactory the operation may be terminated by an anastomosis.

If the circulation in the lower segment is not deemed adequate the following alternatives may be considered:—

1. The establishment of an artificial anus in the left iliac fossa (colostomy). It would appear that this is probably the safest procedure in a large percentage of cases in which cancer is situated in the lower part of the pelvic loop.

2. The rectum is separated from its pelvic surroundings in front, laterally, and behind down to the pelvic diaphragm. Forceps are then introduced into its interior from below, and by means of these it is seized at its upper part and everted through the anal aperture. The greater part of the protruding bowel is removed, leaving a ring of well-vascularised rectal tissue, however, projecting beyond the anus. The upper colon segment, which has been freely mobilised, is drawn down through this inverted cylinder, and the two are united by suture and allowed to recede within the anal aperture.

This procedure is attended by grave risk, and the published records of excision of high rectal cancer by this method reveal a very high mortality. Kraske places it at 40 per cent.

Growths in the middle and upper parts of the pelvic colon.

Lymphatic Area.—Vessels from this colon segment pass directly to the epicolic and paracolic glands, and also to

the intermediate glands lying on the sigmoid arteries and the main group glands on the inferior mesenteric artery.

The "ideal" form of operation in these cases is essentially similar to that just described for cancer in the lower part of the pelvic loop. More of the mesentery will require removal, and the colon will be divided at the commencement of its iliac segment.

As has been stated above, the operation which, as regards its scope, may be termed ideal will probably require to be terminated by colostomy in a large percentage of cases. This would be a great disadvantage, so much so that a reasonable alternative by which the continuity of the alimentary circulation could be preserved, would be deserving of trial. It may be mentioned also that in many instances a purely local excision of cancerous growths in the pelvic colon has been attended by a considerable measure of success.

In the operation about to be described the inferior mesenteric vessels are preserved so as to safeguard the nutrition of the lower segment after the growth has been excised.

Operation.—1. The abdomen is opened in the usual way, &c. The inferior mesenteric vessels are exposed after dividing the peritoneum by which they are covered.

By following the vessels downwards from the left colic branches to the lowest sigmoid artery any visible glands may be stripped away, every precaution being taken to avoid damage to the vessels themselves.

2. The sigmoid arteries are doubly ligatured and divided close to their origin, but the lowest of the series is preserved if possible unless its removal be necessitated by the position of the growth.

3. The descending, iliac, and pelvic segments of the colon are next mobilised in the way already described.

4. The upper bowel section is made at the termination of the descending colon or in its iliac segment. The lower section is in the lower part of the pelvic loop.

Some care is necessary in making this lower section. If the lowest sigmoid artery must be divided at its origin, the level of the lower section in the gut should be below the brim of the pelvis. Failure to adopt this precaution may result in necrosis of portion of the lower segment owing to the cessation of the arterial arcades at the lowest sigmoid artery and the non-union of this vessel with the branches of the superior hæmorrhoidal artery.

Sir Thomas Myles expressed his pleasure at finding Mr. Taylor advocating what he himself had advocated for many years—that was, the absolute necessity for establishing all operative procedure on a solid anatomical basis. A paper of such range could not be adequately discussed in the time at their disposal. He was, however, anxious to know if Mr. Taylor wished them to assume that in the operations under discussion the glands were obviously diseased at the time, or that the removal of the whole area was intended as a prophylactic before there were any visible evidences of glandular disease. Assuming that the intermediate glands were palpably diseased, were they justified in concluding that removal of the area, immediately below the point at which the main branch of the inferior mesenteric artery came off, would guarantee the non-existence of the disease above that point? He concurred with the lecturer as regards the difficulty of effecting an anastomosis when they had to remove a large area starting from the splenic flexure to the upper part of the rectum. But besides the establishing of an artificial anus, there was also possible the attachment of the lower end of the ileum to the upper end of the rectum, or the implantation of the transverse colon on to the rectum, the lip of the colon making a T-shaped junction with the rectum. The difficulty of bringing the mobilised colon into the pelvis was sometimes very great, and sometimes did not exist. The paper had borne out what had been observed clinically—that in certain cases of removal of the

lower bowel, when they got above a certain point, it was better to remove more and get a mobile colon than to bring down the upper part of the rectum.

The President (Mr. Lentaigne) recalled a case which supported Mr. Taylor's remark—that the whip-cord form of cancer of the rectum was most amenable to surgical treatment, and least likely to give trouble by recurrence in the glands at an early stage. The patient was an old lady suffering from intestinal obstruction. He was able to decide that the large intestine was the seat of the obstruction. He opened over the cæcum, and found a lump on the left side of the abdomen opposite where his incision had been made. It seemed to be fairly movable, and he made an intestinal fistula by uniting the cæcum to the lips of the incision. The symptoms of obstruction at once got better, and shortly afterwards he performed a laparotomy, and found the growth, which was a slow-growing cancer of the lower segment. He removed it with about two inches of intestine above and below. An end to end anastomosis was done, and the patient apparently got quite well. Nine years later the patient became ill again, and he found her in a dying state. She had a history of diarrhœa for some months. He found unmistakable evidence of rectal cancer, and she died within a fortnight. The recurrence must have been at an exceedingly slow rate. The most difficult thing of all was to make an early diagnosis, the symptoms were so slight during the time when the cancer could be most safely and easily removed. It was, unfortunately, only when patients began to suffer pain that they came to the surgeon, and pain in the intestine came at even a later stage in the development of the disease than pain in the case of carcinoma of the breast.

Mr. Taylor, in reply, said that even if no glands could be found diseased he thought the complete operation should be carried out. One was not justified always in concluding that because a gland was enlarged it was therefore malignant. If they had reason to believe that some of the secondary glands were involved he was afraid that the hope of radical cure was not great, as the chances were that the main glands would also be diseased. The disease might to some extent be septic, in which case one was justified in giving the patient the benefit of a radical operation. Where the glands were found diseased along the

inferior mesenteric artery, the chance of a radical cure would be absolutely nil. In the case of cancer low down in the pelvic colon, what to do with the upper end was a great difficulty. To leave the patient with an artificial anus in the iliac region was the safest procedure. At the same time one would readily adopt any other method that would restore continuity. The operation of drawing down the bowel was extremely grave, and the mortality was appalling.

NOTES ON AN UNUSUAL FRACTURE OF THE TIBIA AND FIBULA, TREATED BY SCREW-PLATING.

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[Read in the Section of Surgery, March 11, 1910.]

The case which I have the pleasure of bringing under the notice of this Section to-night presents some points of *special* and some of *general* interest.

The general interest lies in a comparison between the results obtained by the old and the newer methods of treating difficult fractures.

By old methods I mean the application of external splints in the form of plaster of Paris or other materials, and in cases when X-rays, applied after reduction, prove this to be efficient and the true alignment of the bones to be restored nothing better is required.

But X-rays show that a large proportion of fractures require a more accurate and more efficient mode of treatment—these are the *difficult fractures*—and it is in this class that the newer methods by open operation are required for success.

(1) Amongst the operative methods may be mentioned the method of *stitching* the fragments together by wire sutures. Those surgeons who have had large experience of wiring bone, and more especially those who view their results regularly in the clear light of X-rays, have learnt that there is a very rapid absorption of bony tissue under pressure, so that *rigid apposition* of the fragments (which is difficult to obtain with

wire stitches at the time of the operation) cannot be relied on for more than a few days, or at most a couple of weeks. After this period X-rays show the wire to have loosened its grip in the bone, and fibrous union (which usually is good) is commoner than bony union where any play exists.

In the long oblique, or spiral fractures of the shafts of long bones, stitching with wire yields better results, if care be taken to drill the stitching holes more or less at right angles to one another, as this method offers better resistance to bending and torsional displacements.

(2) Next comes the method of *lapping* wire *round* the bone, like a fishing-rod splice, which has been used to supplement or replace stitching. This method is not applicable to transverse fractures, and is only at its best in long oblique fractures such as are common in the tibia, and which resist complete reduction by any of the older methods.

In these oblique fractures excellent results are got by a combination of stitching and lapping—good alignment and rapid bony union being obtained.

Stitching with wire also yields good results in the complicated fractures of the humerus just above the elbow joint, where more than two fragments have to be dealt with.

But taken as a whole, with the exception of the long oblique fracture, wire seldom obtains complete rigidity, and less often maintains it. Fibrous union is a common occurrence, and is almost the rule in fractures of the patella and olecranon where it has been most often used.

(3) Next among the newer methods comes the *long screw*, or *nail*, advocated by some surgeons. By long screw I mean a screw of one to three inches in length.

The advent of the long screw marked the first possibility of obtaining absolute rigidity, and at once stopped all pain, and procured rapid bony union.

If care was taken to obtain accurate alignment and apposition at the time of operation, the use of a couple of long screws well maintained this. With this method I have got excellent results in long oblique fractures; but it is better employed in such fractures as the T-shaped fracture of the condyles of femur, or other fractures of the femur near the knee-joint. It is most useful in fractures at the lower end of the fibula or tibia, where separation of these two bones has occurred. But the long screw is at its best when used in fractures of the olecranon, being applied through the tip of this bone in the long axis of the ulna.

By this method, with the minimum of damage to the soft parts, the maximum of rigidity is obtained, and immediate bony union follows. Examples of this I have had the honour of showing here some years ago.

The chief objection to long screws is that it is very difficult to avoid splitting the proximal side of the bone in their application. If this accident happens, of course rigidity of fixation is more or less lost. Another objection is the greater size of the foreign body inside the bone (cancellous or medullary cavity) and the greater damage thus inflicted. However, with care in matching the gauge of drills and screws, and in the use of the screw-driver, I have got very good results by this method, some of which have been exhibited before this section.

(4) The most recent development of screwing is that advocated and so ably designed by Mr. Arbuthnot Lane. I refer to the *short screws* and *long steel plates*, applied directly to the surface of the bone. This method may be termed *internal splinting*, as distinct from any form of

splint applied to the limb external to the skin. Its advantages are numerous. Firstly may be noted that it is the only method which effectively deals with transverse fractures.

Secondly—The short screws are so gauged in length as to grip only in the dense wall of the shaft of the long bones, and not to project into the delicate cells of the medullary cavity. This means less damage and less foreign body irritation when such exists.

Thirdly—Again, by the application of two plates, one in the front, and one on the side of the bone in troublesome cases, absolute rigidity is at once obtained and efficiently maintained. All pain is at once relieved. And if true alignment and apposition have been got at the time of operation rapid bony union and a perfect functional result may be counted on.

These and many other advantages will occur to any surgeon giving thought to the subject, and as we have already had the pleasure this Session of hearing a most admirable paper from my friend, Mr. Lane-Joynt, on the mechanical details of this method, I will not weary you further in the matter.

Asepsis.—There is one point in the surgical technique of this subject which deserves more attention than it often receives, and that is the precise method used for sterilising and maintaining sterile the screws, plates, &c., to be used. These materials when once removed from the boiler are powerless to resist accidental infection from the air or other sources. It is, therefore, of the utmost importance to keep the screws, plates, drills, and screw-driver (or wire when used) boiling all the time in the steriliser until the very moment of application.

If this point be observed there will seldom be any necessity for removing these objects afterwards.

In reviewing a large number of fractures with X-rays, one cannot help being forcibly struck by the enormous improvement in results which attends the operative method in suitable cases.

The older methods left many a patient with, perhaps, a well-shaped limb, but at the same time also some slight limp, disablement, or pain, which, though never severe, still hampered him through life. And I am firmly convinced that many cases of so-called rheumatism following fractures are really caused by want of true alignment in the bone, producing the application of forces at a faulty angle in the joint surfaces above or below the seat of fracture.

Now, turning to the points of special interest in the case exhibited here to-night.

First, we have the extraordinary amount of displacement which was produced, without the fracture becoming compound.

This boy, aged sixteen years, some four months ago was locking up a saw mill. Returning across the floor of one of the lofts he fell through a hole in the floor, in the dark, and plunged blindly and helplessly to the ground beneath, which was covered with rough lumps of stone, bricks, and rubbish. Landing with his whole weight on his left foot, this turned under him, the fracture occurred, and he found the foot strongly everted, with the sole of the foot looking up into his face.

How the fracture failed to become compound under these circumstances can only be explained by the great elasticity of youth. This point is further borne out by the character of the fibular fracture which is apparent in the front view skiagram.

He was at once attended to by Dr. Timmon, of Navan, placed in Cline's splints, and brought up to Steevens'

Hospital next morning. There I saw him the same morning, and at once proceeded to reduce the fracture by extension, counter extension, and manipulations, using all the force that was justifiable.

The result was apparently quite satisfactory so far as could be judged by external examination. There was no shortening, and the limb assumed a symmetrical shape when compared with its fellow limb. Accordingly, plaster of Paris was applied from the toes to a hand's breadth above the knee.

This leads us to a very important consideration. The fractures of the tibia and fibula were apparently quite reduced, and, in the absence of X-ray examination, it would have been justifiable to leave it as it was. This is a case in point proving how important it is to examine and check the results of your treatment by X-rays. The boy continued to complain of great pain in spite of the support given by the plaster of Paris. Accordingly, I made an X-ray examination, the results of which I now hand round. In these two skiagrams, front and side views, you will see the details of the fractures, and you will observe the fact that in spite of an excellent external contour in the limb, the two fractures were very far from being reduced. And if no further efforts had been made this boy would have suffered from great bony deformity, with subsequent pain and disablement.

Eliminating the usual appearance of the ankle joint and epiphysial cartilages at the lower end of the tibia and fibula, at the age of sixteen years, you will observe that while there is little antero-posterior displacement, there is great lateral displacement of both bones. The lower end of the fibula is displaced laterally outwards, through its own diameter, and the character of this fibular fracture is very remarkable, being a sort of "greenstick

fracture," the bone being literally torn into ribbons, thus proving its great elasticity.

Again, turning to the tibia, you see the lower fragment displaced laterally outwards to the extent of one inch by a transverse fracture, which is tightly gripped by its opposing rough surfaces. At the inner end of this fracture there is a loose piece of comminuted bone about the size of a small walnut.

This complicated and unusual state of affairs made me recommend an operation to reduce both fractures by the open method, with the application of steel plates. This was agreed to, and accordingly I had some special plates made to measure in my skiagrams, and drilled with screw holes to fit the fragments, especial attention being paid to avoid any damage to the plate of the epiphysial cartilage, and one of these specially-made plates—*the Y-shaped one*—you will see applied to the tibia in the skiagrams which I now hand round.

I will not delay you with details of the operation; it being sufficient to state that a longitudinal incision was made over the seat of fracture in the tibia, and another over that in the fibula, and the two plates shown in the skiagrams were screwed on in the usual manner. The periosteum was not elevated and was disturbed as little as possible—which I believe to be a point of some importance.

I may mention one point of interest in the operative reduction, and that was the extreme difficulty experienced in separating the surfaces of the tibial fracture.

This was found impossible with the usual bone elevators and levers, and I was obliged to use the special bone lever which I now hand round.

The operation was concluded by inserting silkworm gut drains, and plaster of Paris was applied from the toes to the middle of the thigh.

The after-treatment was uneventful, and followed my normal procedure. Drains were removed on the fifth day, passive motion of the ankle was commenced on the eighth day, and then continued daily.

On waking up from the anæsthetic the boy remarked that he was now free from pain for the first time since the accident. And this observation I find to be universal in all cases where true reduction and alignment are obtained by the rigid application of plates with screws.

The degree of reduction obtained and the true alignment of the fragments by restoring their normal relations to one another are well shown in the skiagrams taken after operation.

These skiagrams are stereoscopic. And this is the best method of taking X-ray photographs when you wish to study accurately the results of treatment.

I thought it might interest the Section to see the progress of callous formation after four months in this case, and accordingly I took two skiagrams, which I now hand round in the negative-form, in this viewing screen. In it you will observe ensheathing callus round both fractures, especially plentiful in the fibular fracture. You will also note that the planes of the fracture have practically disappeared.

In conclusion I wish to thank the members of the section for their great patience in listening to my lengthy and tedious remarks, and may I venture to express the hope that the results in this case may furnish an additional plea for operative interference in suitable cases, provided that a rigid aseptic technique can be guaranteed.

Mr. A. J. Blayney said the case added another to the series showing the excellent results now obtained by the operative treatment of fractures. But he thought there was a slight tendency

to regard every fracture as being necessarily treated by operation, and there were some things still to be said in favour of the treatment of fracture without operation. There was a considerable number of cases in which it was possible to obtain and keep reduction without the use of any mechanical appliances further than external apparatus, and the absence of pain observed after operative measures could also be obtained without operation. In cases, however, like that before them, and in fractures of the femur, there was no doubt the use of splints would not give anything like satisfactory results. On the question of interference with the growth of the bone, if growth continued, a certain amount of longitudinal strain would come, and he was inclined to think that the boy would be better without the plate.

Dr. Stevenson thought Mr. Haughton had hardly laid sufficient stress on the way in which, with an X-ray photograph, it was possible to estimate with accuracy where to make the incision, and where to find the fragments with the least delay. He had assisted at the operation, and before Mr. Haughton cut the skin at all, he was able to show the position in which the plates were to go.

Dr. Stokes asked for some more information on the making of the holes in the shaft of the bone at right angles, also as to the way in which the foot was inverted. He did not see why the screw in the epiphysis should give any trouble.

Mr. Haughton, in reply, said he would consider the taking out of the Y-shaped plate if pain occurred, and the X-rays showed evidence of rarefying osteitis; but he believed that if growth took place it would be very slow, and the pressure on the bone would cause an absorption which would allow the screw to pass upward, and a corresponding deposit of bone would occur on the opposite side. When dealing with a triangular bone by wire, if they put two wires parallel they would have a hinge in which bending displacements were possible, while lateral displacements were not possible; some slight torsion displacement was also possible. To check that, they should have at least two wires passing through the bone transversely, which would give greater rigidity than if the wires were parallel. He did not know in what position the foot struck the ground, but the sole was turned upwards through 180°.

A CASE OF THROMBOSIS OF THE LATERAL SINUS WITH RESECTION OF THE JUGULAR VEIN.

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[Read in the Section of Surgery, March 11, 1910.]

The conditions which most require immediate surgical interference are those which most frequently have been allowed to advance so far that, when the surgeon sees them, they are almost hopeless; and the chance that they will benefit by an operation, at all times grave, is, to the utmost, reduced. The case seen when too late must be familiar to every surgeon. And every one must at one time or another have seriously debated whether a very grave operation could be justified by a very little hope.

The case I am about to report was one of these. The patient apparently had had symptoms of thrombo-phlebitis of the lateral sinus for six days before seeking medical aid. And she was not brought to hospital until septico-pyæmia had already set in. It was the onset of this condition that made me undecided; for, though the thrombosis was clearly evident, it was all the more difficult to estimate how long the pyæmia had endured.

This, as the issue showed, was beyond hope; but as I could not nicely judge of this beforehand I operated, in order that by removing as soon as possible an already disintegrating and infected thrombus, the patient might

have a chance of combating the pyæmia only, which could never be overcome if toxins were to be allowed to continue to infect the general circulation.

The patient was a girl aged nineteen. For two years she had had a discharge from the left ear. This discharge was unaccompanied by pain, and but little occasional tinnitus. The ear was considerably deaf from the time that the running commenced. She stated that eight days before admission she caught cold and had a pain like neuralgia in the head. The running ceased at the onset of this pain. After having had her ear syringed and probed in a dispensary she felt quite sick and dizzy, and had attacks of vomiting and reaching. Every time she would take a drink it caused her to vomit. Nothing remained in the stomach. This continued from Friday, December 31, until January 6—that is, for six days. During all this time the pain in the head was unrelieved. The patient had a temperature of 105° on two days, and several rigors.

In this case, as in many cases, the ear trouble was ignored.

Examination.—The sound of a tuning fork placed at the vertex was not referred to one ear more than another, though one would expect it in uncomplicated conditions of the conducting ear.

When this fork was placed on the mastoid process the bone conduction was shortened to my ear by about twelve seconds. The sound was no longer perceived through the air when the fork was taken away from the mastoid and held in front of the ear. That is to say, Rinne's test or comparison was negative. Nystagmus lasted sixteen seconds when the patient had been turned three times. The pinna did not bulge forwards. There was no swelling behind the ear, and no tenderness. There was no œdema at the posterior edge of the mastoid process. There was absolutely no Griesinger's sign—that is, enlargement of the mastoid emissary veins. There was no perceptible difference in the fee of the jugular veins, and no tenderness over the vein on the infected side, though I must say I did not try hard to elicit this in view of the fact that I was dealing with a case advanced enough to have, if any, a well formed, and probably disintegrating, thrombus. There was no palpable strand or lymphatic

cord in the neck, without which Jansen and Schwartze consider no jugular vein diseased. I did not try the stethoscope test, by which we endeavour to observe if the *bruit* of the jugular is present or not. The head could be moved without pain. There was no stiffening of the cervical muscles. The patient held the head in no characteristic position. There were no eye symptoms nor swellings of the lid. The skin began to grow slightly darker on the second day. The patient was a little hoarse. The urine was acid, charged with albumen and a large quantity of blood. Examination of the drum showed a large marginal perforation. Granulations could be seen in the tympanum. There was no fistula in the posterior meatal wall. The patient's temperature on admission was 98°, but I had a reliable history of its having reached 105° on two separate occasions. Next day it rose to 102.5°, and was followed by a rigor. The pulse ranged between 96 and 116, which, taken in conjunction with the oncoming jaundice, was decidedly high.

Diagnosis.—Though I found not a single localising sign—that is to say, neither œdema over the mastoid, pain in the neck, nor sufficiently marked evidence of irritation of the nerves which emerge from the jugular foramen—I strongly suspected and decided that the lateral sinus was thrombosed. That I did not at once decide may appear strange, but with nothing else but the symptom complex to guide me I was impressed by the fact that a condition similar to this had been seen, and indeed described, by Prescott Hewitt in the *Lancet* of 1861 as a spontaneous cure of lateral sinus thrombosis after rigors and the formation of metastases in the joints and lungs, but it was in reality caused by septic absorption from the spaces of the temporal bone.

Before I saw the case it had been mistaken for some gastric disturbance, and a mixture containing bismuth had been prescribed. Stone in the kidney was also suggested when the hæmaturia appeared. So far from appearing ridiculous these facts are of the utmost importance, for it often happens that in cases of vomiting accompanied by high fever, when attention is not given to the ear as a cause of vomiting, gastric disturbances and far graver sequelæ are liable to be lightly considered or altogether overlooked. Thus it is that acute articular rheu-

matism, malaria, typhoid or miliary tuberculosis are suggested by the symptom-complex of lateral sinus thrombophlebitis.

Meningitis, or brain abscess, though likely sequelæ and concurrent affections, can be differentiated by considering temperature, chills and pulse, or by lumbar puncture as an aid, but as the existence of either of these conditions would point still more strongly in favour of operation they did not enter the question of the present case.

At this time I had not got my apparatus for transilluminating the meatus in working order owing to the difficulty of obtaining a suitable lamp. At present I am using the lamp of a cystoscope until I can get one with an Osram filament capable of taking four or six volts. By placing this little lamp far into the ear one can form an opinion of the character of the mastoid cells—that is, if they transmit the light well, they are pneumatic; if badly, more compact.

Operation.—The mastoid plane was exposed by a curved incision close behind the ear, the periosteum elevated and the ear pulled forward. The periosteum was freed by an incision passing horizontally backward for half an inch, and elevated to expose about $1\frac{1}{4}$ inches of the bone overlying the sinus. This bone was removed by a broad gouge; and, at a depth of about $\frac{1}{4}$ inch, pus under pressure, welled into view. The pus seemed to pulsate, and into the opening whence it came a sucker was introduced.

The bone in front of the sinus was found to have been excavated for the depth of $\frac{1}{4}$ inch undermined, and for $\frac{1}{2}$ inch in an upward direction along the sinus wall.

The vertical portion of the lateral sinus was exposed to view. In colour it appeared whitish grey, with little black spots like petechiae here and there. It was not pulsating, it did not seem to lie in the sinus groove, and it had somewhat same resistance to touch as the eye has under normal tension.

There was some smart hæmorrhage when the highest portion of the exposed sinus was cut. The work of further exposing the sinus was now continued under an irrigation of saline solution. Hæmorrhage ceased, and the upper part of the clot, which was conical, was found to have been thrust down and out through the opening in the sinus, thus effectively preventing any escape

of blood. The jugular vein was exposed in the neck by Mr. Harvey, and tied below the belly of the omohyoid, the muscle was divided and about 2 inches of vein resected as high up as possible. I then split the sinus and detached a clot nearly 2 inches long, shaped like two cones united at the base. When the upper end of the clot was about to be taken out, hæmorrhage was anticipated; and, as soon as the vessel was cleared, the upper portion was plugged with a tampon of iodoform gauze thrust between the wall of the sinus and the upper margin of the incision of the bone.

The radical operation wa sthen completed. A drain was passed through the external auditory meatus as well as a drain placed in the upper portion of the wound, which was but partially closed.

Next day the patient's temperature was normal. On the following day it rose to 104° and dropped to 97° in the evening. She had a distinctly icteric colouration of the skin. There was a rash on the skin of the back. Though there was no sign of compression of the cerebellum, I watched carefully after the operation lest there should arise any abscess formation here, for I have had a case where this developed after operation.

Pathology.—Given acute or chronic mastoiditis of sufficient virulence, the character of the bone between the antrum and sinus is important as a deciding factor in infection. If the cells are pneumatic and separated from the sinus groove by a thin partition only theoretically, infection of the sinus is almost certain to follow. Whereas if the mastoid process is compact it is less probable that sinus infection will occur. But it must be compact and homogenous, for I have seen a large perisinus abscess where the sinus was literally bathed in pus under pressure, and yet the excavated cavity, which contained apparently a drachm of pus, was separated from the autrum by an eburnated partition one-quarter inch in thickness. Whether this was due to the chronic inflammation of the antrum I am unable to say, but though compact it was inadequate to prevent infection of the sinus; but I am

inclined to think that the sinus wall is the most resistant of all these structures.

Reflecting on the operation I must say that I would have now misgivings and hold myself responsible for lack of courage and surgical enterprise had I allowed the patient to die without giving her this one desperate chance.

It would have been not more availing but better surgical technique to have tied the jugular in the neck first, before exposing the sinus and its infected surroundings. I report this case, though unsuccessful, because I wish to benefit more by the opinion and criticism of those members of the Academy who have greater surgical experience, rather than, having selected a more successful case, to be heard with less subsequent discussion and its attendant fruits.

Dr. Dempsey said they should be grateful to Mr. Gogarty for showing a case which was not an operative success. Lateral sinus thrombosis was much more commonly met with since the radical mastoid operation had come into vogue. The great difficulty was the question of diagnosis in certain cases, and in dealing with such he thought they should be guided by what they met with when treating the case. He had seen a case in which the diagnosis was typhoid fever that developed some discharge from the ears. He cut down on the mastoid, and found extensive bone disease. He did the ordinary operation, and the patient was much better for several days. Then came a temperature of 105° and sharp rigor, suggesting lateral sinus disease. He reopened, but found things as at first. The same symptoms recurred for three weeks about four times a week, but further operation was refused, and yet the patient got steadily well, and was now in perfect health. In another case, in which the radical mastoid operation was performed, the patient also developed a high temperature and rigor. The lateral sinus was exposed, and showed a surface greyish, and breaking down, with the emission of pus. The jugular vein was tied in the neck, and the sinus was freely opened

with free hæmorrhage. The patient died, and the trouble was found to be a cerebellar abscess. If one met with a vein clotted down in the neck, it should be cut down on and the vein excised; but if one found the sinus filled with clot in the region of the mastoid, the question arose as to whether the internal jugular should be tied at all or not. Opinions were about equally divided. Cases in which it was not tied had done just as well as if the ligature had been effected. But the test was hardly a fair one, as the cases of tying were practically desperate cases which were given a last chance.

Dr. Harvey sympathised with Mr. Gogarty in his brave endeavour to save the patient's life, but the patient had been infected several days before the operation, and the diagnosis was very difficult. Mr. Gogarty's procedure was very wise, and he would like to know what were the indications as to when the internal jugular should be opened.

Mr. W. S. Haughton said it was from such cases very often that they learned most. He had operated on five cases of suspected thrombosis, in three of which the condition was found and death followed. In the other cases there was no apparent thrombosis, and death followed. He did not ligature the internal jugular, but he was inclined to think it should be done oftener than it had been done. In a case which was not septic, he adopted a trephine opening, and cut into the lateral sinus. Severe hæmorrhage followed, and he was obliged to plug. Six days later he reopened and removed the plug, and no hæmorrhage followed. He had, however, given up the trephine, as he found the broad gouge did the work more safely.

Mr. Wm. Taylor said he had seen the swelling which was found along the jugular vein to be due, not to an infection of the vein, or thickening, or clot, but to an infection of the glands. Where they could easily recognise on palpation the cord-like swelling described in the books, it was too late; but there was one chance in five hundred which should be given to the patient. Where there was the smallest suspicion of any intra-cranial complication, operative interference should be undertaken at an early stage. In the combination of two intra-cranial complications due to ear disease, the difficulty of diagnosis was greatest, and he believed that septic sinus infection predominated. He

had seen a case in which the phenomena of cerebellar trouble did not manifest themselves until some twenty-four or thirty-six hours after the lateral sinus had been opened. Cases of sinus infection, where there was not a clot, were, he felt, far more dangerous than those in which there was, and he would be inclined to ligature the jugular, even though the infection might be carried by other channels. He would have no hesitation in doing so where there was clear evidence that the sinus was infected; where there was a doubt, he would expose the mastoid and the lateral sinus, and deal with the condition as he found it.

Mr. Pearson asked what were the indications for operating on obvious ear cases, where pus was found under tension in the mastoid without any definite evidence of other complication at the time. He recalled a case in which the radical mastoid operation was done. After the operation the patient appeared to be very well, but temperature rose later to 103°, and a blood-count showed a marked leucocytosis. The patient became very dull, and answered questions slowly, but there were no other symptoms. The patient was reopened, and a large tempero-sphenoidal abscess was found with pus under very high tension. He felt certain the abscess was present at the first operation, but that it had passed the stage at which it gave the symptoms for which they were on the lookout.

Mr. Gogarty, in reply, referred to the question as to when the jugular vein should be resected in the case of thrombosis of the lateral sinus. In his opinion, although he knew that many thrombi had been successfully removed without ligaturing or resecting the vein beforehand, it was better to ligature the vein if it was decided to open the sinus when the surrounding parts were septic; and to resect the vein in case of thrombosis of the jugular bulb, or whenever it seemed probable that the vessel wall might be itself a carrier of infection.

TWO CASES OF NEPHRECTOMY.

By R. ATKINSON STONEY, M.B. Univ. Dubl., F.R.C.S.I.;
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[Read in the Section of Surgery, April 22, 1910.]

The following two cases of nephrectomy are, I think, of sufficient interest to merit recording:—

Case I.—J. B., male, twenty-eight, while riding a bicycle on Monday, August 2, down hill, collided with a man, and was thrown off his bicycle and fell on his right shoulder. He fell clear of his machine, and was not conscious of striking the ground, except with his shoulder. He got up immediately, but fell down again, and was unconscious for about ten minutes. He then went home, and half an hour after the accident he vomited. For the next week he remained very quietly in bed, suffering great pain under the ribs on the right side. The pain subsided somewhat, and the next week the patient was able to sit up in bed, and by the end of the week the patient was up and about, though he did not return to work. On Thursday, August 19—that is, the seventeenth day after the accident—the patient for the first time noticed blood in his urine. He brought a specimen to hospital with him, and, as it was found to contain a large quantity of blood, he was at once admitted to the Royal City of Dublin Hospital. The patient was quite positive that this was the first time since the accident that he had passed any blood. For the first few days the patient was in hospital he passed what appeared to be nearly pure blood in large quantities, but on the 22nd he had a rigor, his temperature rising to 104°, and he had retention of urine; on passing a catheter the bladder was found distended with clotted blood. The temperature fell the next day, but remained somewhat irregular. A few days later a catheter had to be passed again to break up clots in the bladder. On the 28th the urine was nearly clear, and remained so for two days, but on the 31st,

while the patient was being moved in bed to have his back rubbed, he felt a sudden pain in his right side, and the next urine passed contained a large quantity of blood and some clots. On the 4th of Sept. the urine was segregated by means of Luy's segregator; the urine from the left side contained no blood, that from the right appeared to be nearly pure blood. Since admission the patient had been passing from 40–85 ounces of urine a day, an average of 56 ounces for the sixteen days he had been in the hospital. The patient was wasting rapidly, and had become very anæmic. On the 6th there was only a trace of blood in the urine, and the temperature rose again to 104° on the following day. The patient's pulse had become much weaker during the last two days, and it was decided to operate on the 8th. A lumbar incision was made on the right side, and the region of the kidney exposed. A thick layer of partially organised blood clot behind the kidney was divided and the kidney reached. In attempting to peel the peritoneum off the anterior surface of the kidney a rent was made in it, and it was found impossible to separate it, so an incision was made through the capsule of the kidney, and this was peeled off, with the peritoneum, towards the hilum. In enlarging the wound, so as to allow access to the upper pole of the kidney, the pleural cavity was opened, although the incision was kept well below the angle between the last rib and the outer border of the erector spinæ; the opening was at once plugged with gauze held in place by a retractor. The kidney was now freed and found to be almost divided in two near the level of the hilum, and the lower half to be largely disintegrated and merely represented by a mass of blood clot. The pedicle was divided from above downwards, the vessels being clamped one after another as they were exposed and cut; there was comparatively little bleeding during this process. The vessels were then tied with catgut, and, to stop some oozing, a few deep catgut sutures were used to unite the anterior and posterior parts of the kidney capsule. The rents in the peritoneum and the pleura were stitched, a piece of gauze passed down to the remains of the kidney capsule, and the muscles sutured with catgut and the skin with silk-worm gut. The patient was given two pints of saline subcutaneously during the operation. The patient made

an uninterrupted recovery. The gauze was removed on the fifth day, and the stitches on the seventh; the wound healed rapidly. There was no more blood in the urine after the day of operation, and the cystitis, which had been present before operation, gradually cleared up. On the day of operation the patient passed 42 ounces of urine, and on the following day only 24. But on the next three days over 50 ounces was passed daily, and from that to the time he left hospital the average was 43 ounces. The patient left the hospital on October 9 for the convalescent home, and when seen in the middle of November he was quite well, and had put on over a stone in weight.

The history in this case is hard to explain. Why, when the kidney was so severely injured, was there no blood in the urine for more than a fortnight? Why, when the kidney was nearly torn in two, and the calyces opened, was there no extravasation of urine and the formation of a perinephric abscess? How was so much damage done to the kidney, so that the lower half of it was pulped, by a fall on the shoulder? And, lastly, why, with such severe injury, was there nothing more than pain in the side? The following is the probable explanation of the course of events:—When the patient fell on his shoulder the trunk was forcibly bent to the right and the kidney, especially the lower pole, was crushed between the last rib and the crest of the ilium, blood was extravasated into and around the kidney, and clotted in the ureter, the injury to the kidney was so great that suppression of urine occurred on this side, so preventing urinary extravasation. When the patient got up and began to move about at the end of a fortnight the clot in the ureter was dislodged, and also those in the torn vessels and hæmaturia, for which the patient was admitted to hospital, occurred; about this time, also, the remainder of the kidney began to secrete again. On at least two occasions during the patient's stay in hospital before operation the ureter again

became blocked by clot and the hæmaturia ceased temporarily. The adhesions formed made the operation difficult, and precluded the possibility of isolating and clamping the particle before removal. Fortunately, the opening of both the peritoneal and pleural cavities were not followed by any unfavourable results. The pleural reflection was abnormally low, crossing the last rib below where the latter was met by the outer border of the erector spinæ, instead of above this point, as is the normal arrangement.

Case II.—E. B., schoolmistress, aged twenty-seven, noticed blood in the urine on May 14 last after a severe chill. She also complained of some pain in the left side. She remained in bed for three months, and the urine, which was frequently tested, always contained some blood. Tubercle bacilli were also found. During the months of August and September the patient was up and went about a little ; she had pain in left side and back, never in the groin or thigh, and never noticed pain when passing water. Since the beginning of October there has been more blood in the urine, and the pain has been worse. She was admitted to the Royal City of Dublin Hospital on October 6. She was a well-nourished, healthy looking girl, and had increased 5 lbs. in weight since the beginning of August. There was slight pain and tenderness on pressure over the lower pole of the left kidney, which was thought to be palpable. The urine was red in colour, containing blood in large quantity and pus. Tubercle bacilli were demonstrated in the urine. On October 9 the patient was anæsthetised with ether and the urine segregated with Luy's segregator ; the flow of urine was very slow, and the amount collected small, but that from the right side contained no blood, while that from the left did. On October 16 the urine was again segregated ; this time the patient was not anæsthetised. A pint of weak tea was given half an hour before and a pint of milk and soda water was given during the procedure. During about 40 minutes 11 drams of urine were collected from the left side and 8 from the right. The urine from the right side came in gushes of three or four drops at a time, was quite clear, and contained no pus or blood ; that from

the left came in gushes of five or six drops at a time, contained blood and pus, and tubercle bacilli were demonstrated in it under the microscope. From the time the patient entered hospital till operation, although the patient was kept absolutely quiet in bed, the amount of blood in the urine did not diminish, but was always considerable. Nephrectomy was therefore performed on Tuesday, October 26. The kidney was exposed by the lumbar route. It was found to be very freely movable and somewhat larger than normal, and a cavity the size of the top of one's finger could be palpated in the upper part. The pedicle was clamped and the kidney removed. Unfortunately in transfixing the pedicle preparatory to ligaturing it, the blunt needle pierced one of the branches of the renal vein, and there was some difficulty in stopping the hæmorrhage. The vessels were finally tied separately with silk, but a clip had to be left on one bleeding point. The clip was removed on the fourth day, and the stitches on the eighth. The portion of the wound which was not sutured on account of the presence of the clip healed rapidly, and the patient left hospital for the convalescent home on December 1. There had been no blood or pus in the urine since the operation, and a microscopic examination failed to demonstrate the presence of tubercle bacilli. Before operation the patient was passing a daily average of $45\frac{1}{2}$ ounces of urine, and although for the first six days following the operation the average was only $22\frac{1}{2}$ ounces a day, the amount then rapidly increased, and the average for her stay in hospital after operation was $48\frac{1}{3}$ ounces—a little more than the amount excreted by both kidneys before the operation. On splitting the kidney open it was apparently normal except for a cavity in the upper pole, which practically corresponded to one of the pyramids ; it was smooth-walled, and had a capacity of about half a dram. Over a small area the wall was slightly nodular, and suggested the presence of tubercles. Sections were made of portions of the wall and examined, but the microscopical appearances were not definite, though very suggestive of tubercle. No giant cells were seen, but considerable round-celled infiltration and close-packed masses of round cells here and there.

This case also is peculiar from the disparity between the symptoms and pathological findings. For here,

though the disease had lasted for about six months, and was marked by constant presence of blood and pus in large quantities, the disease was limited to a very small portion of the kidney. If a partial nephrectomy for tubercular disease is ever justified, which is denied by some of the best authorities, this certainly would have been an ideal case for such treatment.

When seen on her return, after a fortnight's stay at the convalescent home, the patient was looking well, and had put on eight pounds in weight.

Mr. Seton Pringle said he was particularly interested in the second case, as he had had two cases of a similar nature which had rather puzzled him. In both cases the kidney was exposed, and in neither was he able to demonstrate tuberculosis. Both made perfect operative recoveries, but one was still—eighteen months after operation—complaining of pain and frequency, and passing some pus; and the other, seen two months ago, was practically the same as before operation, and still passing purulent urine. It was important to know when to take the kidney away or when to leave it behind. He thought the cystoscope was a great advance on the segregator in dealing with such cases. He would suggest that in the case of the tubercular kidney it was the ureter that was chiefly involved. From the naked eye appearance the kidney would not be called tubercular. He would like to know what material Mr. Stoney used for ligature of the ureter and vessels. He personally had had trouble with silk coming out of the wound afterwards.

Dr. Stokes said the first case seemed to be a very rare lesion, and the question was when they should operate. He wished to know if Mr. Stoney had any rule on the point. He had had trouble in using the segregator, the methylene blue which he injected returning down the other side. He would like to know if in the tubercular case there was any other focus in the body.

Mr. C. A. Ball said he had met several cases which bore out Mr. Stoney's contentions in regard to his second case. He had found the segregator very useful in some cases, and had found

it to work perfectly well, but if one used a cystoscope the segregator would rarely be required.

Mr. W. S. Haughton said the ruptured kidneys he had had to deal with generally showed hæmorrhage from the start. The weight of opinion seemed to be in favour of removal, and he would like to know if Mr. Stoney had ever tried suturing a ruptured kidney. He had once done so on a kidney which had three tears. He stitched with thick catgut, and drained with satisfactory results. He had once been compelled to leave in a kidney which was proved to be tubercular. There were three separate tubercular abscesses, but it was impossible on account of inflammatory adhesions to remove it. The case did fairly well, and the lady engaged in her household duties, but a little pus was still being discharged.

Mr. J. Lentaigne said his experience had been that if there was a condition in which the treatment by injection of tuberculin was really valuable it was in tuberculosis of the genito-urinary organs, especially the kidney. He had seen cases too ill for operation which improved to a wonderful extent although the injections were carried out without being controlled by the opsonic index. He did not think they ought to operate in such cases without giving tuberculin a trial.

Mr. Stoney, in reply, said that against the idea of the ureter being involved was the fact that although he did not remove any portion of the ureter, there was a complete cessation of all symptoms from the time of the operation. His impression was that in removing the kidney manual pressure was put on the cavity, which was thus completely emptied, any tubercular material in it being driven out. He considered catgut the best material in such cases: he used the catgut prepared by the iodine method, and had always found it sterile. He had found the segregator absolutely successful in many cases. He thought the mistake in the use of it was in the position of the patient. If the patient was put sitting up in a gynæcological chair so that the legs were nearly at right angles with the body, there was no possibility of urine passing from one side to the other, unless the bladder became over-distended. In most cases where there were symptoms of extensive injury, he thought the sooner operation was done the better. There were no other tubercular foci

found in the body in the record case. The hæmaturia was apparently in many such cases one of the first and most important symptoms of tubercle in the kidney; and he thought they might take it for granted that in a young patient severe hæmaturia which was not affected or relieved by continued rest was almost pathognomonic of tubercle. He had not seen a case where suturing was possible, as his cases had always been so extensively injured that it was out of the question. The results which he had obtained with tuberculin had not been satisfactory: there seemed to be a great tendency to recurrence.

COXA VALGA: WITH REPORT OF A CASE ASSOCIATED WITH ANKYLOSIS OF BOTH HIPS.

By SETON PRINGLE, M.B., F.R.C.S.I.;
Surgeon to Mercer's Hospital, Dublin.

[Read in the Section of Surgery, April 22, 1910.]

The condition of coxa valga is one about which little or nothing was known until the advent of expert skiagraphy, and as these cases are comparatively rare, I thought it would be of interest to the members of this Section to bring an example of the condition before them to-night, more especially as I believe this to be the first case to be reported to the Academy. A description of the deformity has not yet found its way into the text books, and few monographs have appeared on the subject; the best account in English is that of Tubby, (1) whose classification I have followed in the subsequent remarks.

Coxa valga is, of course, the reverse of coxa vara, the essential part of the deformity being that the normal angle of inclination of the axis of the neck of the femur to the axis of the shaft is opened out. This angle normally varies from 125°–128°, so that if it, in a given case, measures more than 128° the condition is one of coxa valga, whereas if it is less than 125° we have coxa vara. It has not yet been determined whether there is an alteration in the curve of the neck of the femur itself in this deformity analogous to the curve convex forwards found in coxa vara. This point can only be decided by *post-mortem* examinations on numerous cases.

As regards the ætiology of this condition, we may divide the cases into Congenital and Acquired.

A. Congenital. Skiagraphy has taught us that coxa valga is commonly associated with congenital dislocation of the hip. The valgus deformity in these cases may be either *primary*, and, therefore, the cause of the dislocation, the neck of the femur being too straight to allow of proper articulation between the head of the bone and the acetabulum, or *secondary*, and caused by the lateral thrust of the iliac bone when the child begins to walk. Whether we ever get congenital coxa valga without dislocation is at present a moot point, into the discussion of which we have not time to enter to-night.

B. Acquired. In considering the causes of acquired coxa valga we must remember that the normal shape of the head and neck of the femur depends on a nice adjustment of certain forces, viz., the weight of the limb, the pressure of the body weight, the pull of the pelvi-femoral muscles, and the amount of resistance the bone is able to offer to pressure. It is, of course, clear, that, apart from some process of bone softening, the resistance of the bone to alteration of its shape will be sufficient, except in childhood and adolescence, to counterbalance any disturbance of these forces, and so to prevent deformity. Consequently, the great majority of acquired cases are found in children and young adults.

These acquired cases may be divided into four classes :—

(1) A class caused by the traction of a pendant shortened limb, or by the non-use of a limb for a long period. These cases are found in children associated with such a conditon as infantile paralysis of the lower extremity. Here we have the pull of the weight of the limb, the loss of the pressure due to the transmission of the body weight, and the absence of the normal

pull of the muscles round the joint, all tending to cause a straightening out of the neck of a femur already atrophic from disuse.

(2) A static or functional form, caused by alteration in the shape of the bone compensatory to some other deformity. It has been found by experience that if in children the limb is, from any cause, held for a lengthened period in a position of adduction we get as a result coxa vara of the neck of the femur, whereas if the position of abduction is long maintained we get coxa valga resulting. For example, genu valgum of one knee causes coxa vara of the same side and coxa valga of the opposite side, or, again, if one leg is shorter than the other the short leg is abducted to enable the child to walk, and coxa valga on that side results, or, again, if there is a curve convex outwards in the shaft of one femur, due, for instance, to rickets or the malunion of a fracture, we again get coxa valga on that side.

(3) A form associated with any process of bone softening, such as rickets or tuberculosis, accompanied by false adjustment of the forces normally acting on the joint, *e.g.*, it is quite conceivable that coxa valga would be produced by prolonged excessive extension in a case of tubercular hip.

(4) A traumatic form, due to malunion of an impacted fracture of the neck or separated epiphysis.

Signs and Symptoms.—As regards the signs and symptoms, here is not much to be said. Pain is not a marked feature, although it is sometimes present early in the case. The leg is abducted and rotated outwards, while there is marked limitation of adduction and internal rotation. In unilateral cases there is lengthening of the limb to the extent of 2–3 cms., which is not compensated for either by tilting up of the pelvis

or by adduction of the limb. The gait in unilateral cases is limping, with the trunk inclined to the bad side, but, in bilateral cases, is rolling and unsteady, being rather similar to that of double congenital dislocation of the hips. This unsteady gait is due to the fact that the head of the bone, on account of the abnormally straight neck, cannot articulate properly with the acetabulum. Again in a bilateral case the patient stands on one leg with great difficulty, and in a unilateral case the patient, when standing on the affected leg, inclines the body to that side, a position just the reverse of the Trendelenburg's sign in single congenital dislocation of the hip. Finally, the great trochanter is flattened and often below Nélaton's line, and in unilateral cases there is some scoliosis of the lumbar spine, convex to the affected side.

Diagnosis.—The diagnosis largely rests with careful skiagraphy, although the position of the limb (abducted and rotated out with limitation of adduction and internal rotation, but without compensatory adduction) is said to be pathognomonic. In taking X-ray photographs of this condition it is essential that the tube should be directly over, or even outside the joint, for a photograph of a normal joint taken so that the rays pass in a downward and outward direction while the limb is everted will show apparent straightening out of the neck of the femur.

Treatment.—Before discussing the treatment of coxa valga it is well to point out that some examples of this deformity are discovered accidentally, as there may be no definite symptoms. Indeed, in these so-called physiological cases the angle of inclination may measure as much as 140°, instead of a normal maximum of 128°.

The methods to be employed in cases requiring treatment are mainly operative. Blanchard (2) advises cuneiform osteotomy of the neck of the bone ; Galeazzi (3) linear osteotomy at the base of the neck, and he quotes successful cases ;

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Fig. 1.

while Allison (4) advocates subtrochanteric osteotomy as being sufficient, and by far the most simple procedure. Tubby (1) suggests, but does not advocate, the taking of a wedge of bone, with the base of the wedge on the inside, from the subtrochanteric region, *i.e.*, the reverse of the operation often performed for coxa vara. As regards non-operative methods, David (5) succeeded in improving a case by placing the limb as far as possible in the adducted position, and fixing it so placed in plaster of Paris. Tubby also tried this procedure, but without success. The wearing of a boot with a high sole will sometimes improve the gait, and even lessen the angle of inclination by altering the lines of force acting on the neck of the bone. I performed double subtrochanteric linear osteotomy in the case before you, the details of which I now propose to give you.

Case.—K. S., a little girl, aged five, was sent up to me by Dr. McQuaid, of Cootehill, with the history that a year previously she had been seriously burnt as a result of her clothes catching fire. Before the accident she was able to walk normally, but when the large superficial burn was healed it was found that she was unable to extend her thighs, and locomotion was greatly interfered with. The burn took nine months to heal, and for most of that time she used to sit for the greater part of the day in a chair at the cottage fire.

On examination I found her to be a plump, well-developed child, with an abdomen very suggestive of rickets, some beading of the ribs and enlargement of the lower epiphyseal line of the radius. The outer side and front of the right buttock, and the right lateral aspect of the trunk as high as the lower margin of the ribs, was covered by a thin, red, papery scar, which also encroached on the right iliac region of the abdomen. This was, apparently, more or less superficial, and not extending deeply into the muscles. Both hip-joints were ankylosed, only allowing a range of movement of about 10°. Both thighs were flexed at an angle of 60° with the horizontal, were abducted 25° from the axial line of the body, and everted to the extent of 45°. Both tro-

chanters were greatly flattened. In attempting to walk she inclined the body very far forwards with a deep lumbar lordosis. She was greatly disinclined to walk without a helping hand, and her gait was unsteady and swinging. The accompanying photograph (Fig. 1) shows her position very well when standing. Skiagrams were taken with the rays passing downwards and outwards, vertically downwards, and downwards and inwards through the hip-joint, and showed in all positions a great straightening out of the neck of the femur, so that even in those taken with the tube outside the joint the angle of inclination is about 170°. The outline of each joint is also rather "furred" in the photographs. On examination under an anæsthetic it was found that no improvement could be made in the position of the limbs, so that finally I decided to do a double sub-trochanteric asteotomy. This operation was performed on January 10th, and the child was placed in a double Thomas's splint and extension by means of a two-pound weight applied to each limb. To-night, three months after the operation, I present the child for your inspection, so that you may judge of the result. You will notice that she is now able to stand erect (Fig. 2), and on a much narrower base than formerly, so that the flexion and abduction have been almost completely corrected by the operation. Of course she will never be able to walk freely, as her hip joints are still ankylosed, but she is already able to get about quite satisfactorily, and no longer inclines the upper part of her body forward in the marked manner shown in the first set of photographs.

As regards the ætiology of this case, I am inclined to look on the ankylosis as being due to that form of osteo-arthritis which Howard Marsh (6) has called attention to, which is caused by the absorption into the circulation of septic products. As pointed out by Marsh, acute symptoms may be almost absent, and may easily have been masked in this case by the pain of the healing burn, and the quantity of toxins absorbed from the large area involved in the burn must have been far more considerable than that in some cases mentioned by him, for he quotes a case caused by absorption from a small alveolar sinus. With regard to the coxa valga, I believe it to be an

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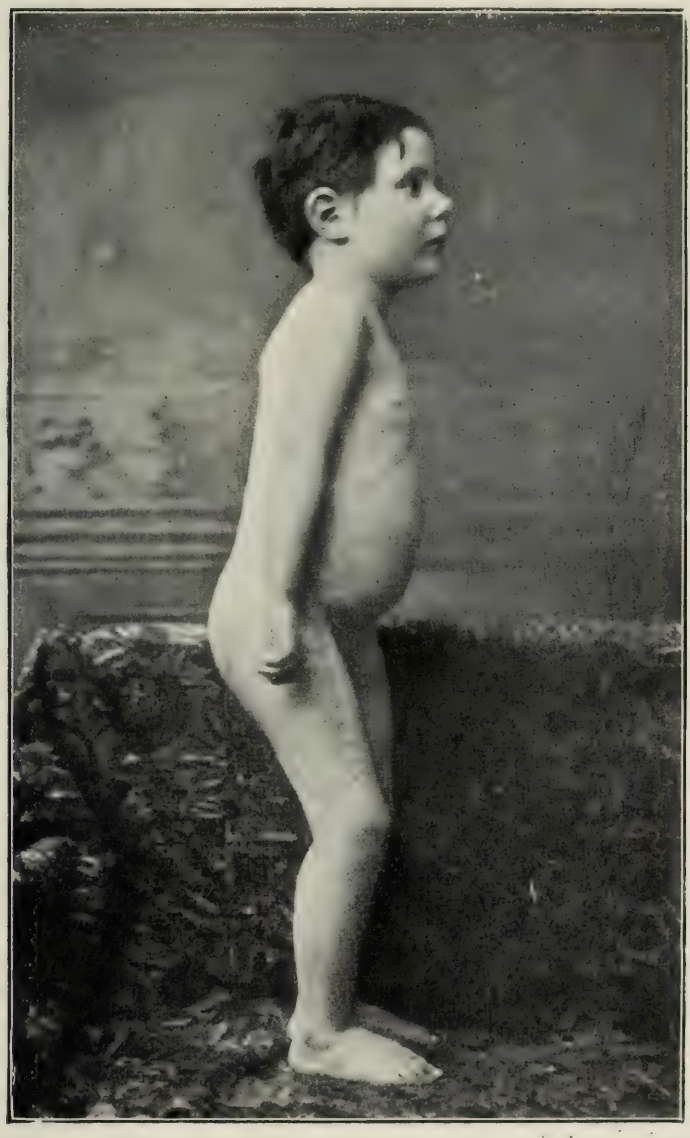

Fig. 2.

acquired condition in this case, for here we have a patient who not only was young enough to allow of moulding of the bones, but who also was the subject of rickets and of a quiet osteo-arthritis, and in whom the normal forces acting on the hip-joint were, for the period of nine months, greatly disturbed by her not being able to walk, and by the sitting position assumed for the greater part of each day.

In conclusion, I desire to thank Dr. W. G. Harvey, who kindly took great trouble in getting the first set of skiagrams, and also Dr. H. W. Mason, who took the skiagrams of the post-operative condition.

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Mr. J. Lentaigne said the condition was a very rare one, though it had doubtless been met, but not recognised. Its discovery was entirely due to the use of radiography.

Mr. Stokes enquired as to what joints were most commonly affected in cases of septic absorption.

Mr. Blayney said they all felt they knew very little about the subject, and for that reason there could be little in the way of discussion. His attention had been called to the fact that, in cases of congenital dislocation, the skiagrams seemed to show the neck as being a direct prolongation of the shaft.

Mr. J. Lentaigne suggested that the ankylosis of the hip joints occurred first, and with continued growth the position of the hips would tend to produce the condition. There must be something tending on both sides more or less equally to produce the deformity.

Mr. Pringle, in reply, said the arthritis did not select any special joints. He thought the position of the child had determined the site of the disease.

NOTES ON A CASE OF UN-UNITED FRACTURE OF THE TIBIA TREATED BY WIRING.

By WALTER C. STEVENSON, M.D., B.Ch., UNIV., DUB.;
Surgeon, Orthopædic Hospital, Dublin;
Assistant Surgeon, Steevens' Hospital, Dublin.

[Read in the Section of Surgery, April 22, 1910.]

This case is of interest on account of the almost complete absence of bony callus four weeks after fracture, and the subsequent rapid repair that followed the fixing of the fragments by open operation. The history is as follows:—

While the patient, a man, aged twenty-seven, was riding, the horse suddenly slipped and fell, pinning the rider's leg against the ground, and causing, partly by a torsion of the foot, an oblique or spiral fracture of the right tibia and fibula. The fracture would almost certainly have been compound but for the tight legging which was worn at the time of the accident. The patient did not suffer much pain until the legging was removed on reaching home. The leg swelled rapidly, and bullæ formed. There was no displacement in the fibula, the fragments being interlocked; and as this fracture gave little trouble it need not be discussed further. The lower end of the upper fragment of the tibia was seen and felt as a prominence under the skin in the lower third of the leg. On account of the precarious condition of the skin, and the patient's refusal to take an anæsthetic, the leg could not be dealt with adequately at the time of the accident. It was therefore placed as securely as possible in a box-splint. As the patient was rather restless, and nervous about his condition, and on account of his experiencing a good deal of pain, he was constantly shifting the bandages, and moving the leg into what he considered a more comfortable position, so that proper rest was not possible. He also suffered from dyspeptic symptoms, his temperature

sometimes rising to 100° F. As there was no mitigation of pain, and little attempt at union after four weeks, Dr. Bouchier Hayes, who was attending the patient in the country, persuaded him to come to Dublin for an X-ray opinion, and any further treatment that might be necessary. On his arrival I found that in some positions of the leg the lower point of the upper fragment was almost piercing the skin on the inner side. This point of bone could be made slightly less prominent by rotating the foot, but the manœuvre caused great pain.

Skiagrams which I took the next day—that is, twenty-nine days after the accident—showed an oblique fracture of the lower third of the tibia, about 3 inches long and with $1\frac{1}{4}$ inches overlapping of the broken bone. The upper fragment was displaced inwards the whole breadth of the bone. There was only the smallest trace of bony callus at the upper point of the lower fragment. This absence of any attempt at repair after four weeks, especially as the patient continued to suffer from flatulence, vomiting, loss of appetite, and sleeplessness, made one suspect that there was deficient power of laying down lime salts.

Briefly, the line of treatment I adopted was—first, to fix the broken surfaces of the bone in good apposition, and, secondly, to promote the speedy consolidation of the fracture by permitting the use of the limb as much as possible, with due regard to the amount of osseous repair at the site of lesion. I found my series of X-ray examination of the greatest possible service as a guide in the treatment, and as a safeguard against allowing the patient to do too much. Internally, the patient was given 15 grains of lactate of calcium and a drachm of syr. hypophos. co. three times daily. I may add here that the dyspeptic symptoms disappeared shortly after the patient was allowed up, and was satisfied that the leg was progressing favourably. In consultation with my colleague, Mr. Haughton, and my friend, Dr. Bouchier Hayes, I decided, in accordance with the patient's wishes, to attempt reduction under an anæsthetic by traction and manipulation, and fixation by plaster of Paris bandages. This procedure I considered justifiable on account of the mobility of the parts. But owing to the shortening of the muscles the fragments could only be placed in partial apposition, as was seen in the skiagram I took through the plaster on the following day.

After a consultation with our President (Mr. Lentaigne), Mr. Haughton and Dr. Bouchier Hayes, it was decided to plate or wire the fragments. When it was pointed out to the patient that this would give him the most useful limb and in a shorter time, he agreed, being anxious to return to his occupation as soon as possible.

On January 28, thirty-six days after the accident, I operated with the valuable assistance and advice of Mr. Lentaigne, Mr. Haughton, Dr. Hayes, and also Dr. Kirkpatrick, who gave the anæsthetic for the second time.

The incision was made, starting well back on the inner side of the tibia about $1\frac{1}{4}$ inches below the projecting bone. It curved forwards and then ran straight upwards, just external to the crest of the tibia, and curved back over the subcutaneous surface. A flap consisting of all the structures down to and including the periosteum was dissected off from the inner side of the tibia. With an elevator the periosteum was easily separated from the other surfaces of the tibia, so that about 4 inches of naked bone was exposed in the wound. There was a little muscular tissue between the fragment at the anterior and upper part, without any attempt at union.

The tendo Achillis was now divided subcutaneously with a sharp-pointed tenotome. Even then the fractured surfaces could not be got into proper apposition. The lower end of the upper fragment overlapped the lower fragment on the subcutaneous surface of the tibia to about $\frac{1}{4}$ inch. A quarter of an inch of bone was therefore sawn off parallel to the line of fracture and from the subcutaneous surface alone. When the bones were put in position they lay well together, and in good line along the inner surface of the tibia; but the long spicule of bone which formed the upper end of the lower fragment had on that account, of necessity, to project outwards. I decided to leave this as a scaffold for callus, though recognising at the time that one objection to not removing this spicule was an æsthetic one—from an X-ray point of view.

I had now to make up my mind whether to use plates, screws, or wires, or a combination of these. Wiring seemed the best method of securing the fragments above. I drilled the two fragments transversely, and passed a soft iron wire through.

The ends were crossed and tightened in front, then passed completely round the tibia, and finally twisted and cut off on its inner surface, The wire held the fragments so well together that it seemed advisable to repeat the same procedure. This was done about an inch lower down, the drill hole being made in a slightly different direction. The fragments were now found to be so firmly held in position that it was unnecessary to plate the bone, as I had originally intended. On removing the tourniquet there was some oozing, but no vessel required ligation. A small rubber drainage tube was passed down to the bare bone through an incision in the centre of the flap. The only stitches used were the interrupted cutaneous ones, of silkworm gut, which also included the periosteum of the flap. The limb was put in plaster to above the knee, the foot being at right angles to the leg and the knee slightly bent. The plaster was cut down the centre and also across the inner side to form a window.

The patient's temperature never rose above 99.4° F. The drain was moved on the second day; and he was allowed on to a couch on the fifth day. The wound healed completely by first intention.

On the fourteenth day after operation the patient was able to get about on crutches with the injured limb off the ground. The plaster was removed daily for massage.

A skiagram taken on the twenty-third day showed a fair amount of bony callus diffused like a cloud over a considerable area round the seat of operation. In the later skiagrams this cloud appeared more and more concentrated at the site of the actual break. The wire encircling the bone could be seen to tunnel the callus.

As the skiagrams showed that neither the wires nor the fragments had moved since the operation, the leg was encased in a strong plaster splint, which did not include the knee, and was applied so as to transmit the weight of the body from the ground to the tuberosities of the tibia. At the beginning of the fourth week the patient was able to walk in the garden with the aid of two sticks and the

plaster. He declared he walked five miles one day. This plaster was worn for a fortnight, and then cut down and removed to permit the limb to be massaged.

To give the patient more liberty to get about with safety a removable plaster case was now applied. The roller bandages were applied over a carpet slipper and a Jager stocking. Embedded in the plaster were four steels, two in front of, and two behind, the malleoli. The two anterior went down over the dorsum of the foot; the two posterior below the heel, the inner one being bent across underneath the heel. The four steels extended above to, and were moulded against, the tuberosities of the tibia. The plaster was cut down on either side on to two strips of zinc sheeting, previously placed under the stocking. The plaster was hinged below the instep by the sole of the slipper, which was not divided. After being cut the plaster was fastened round the limb again with buckles and straps, and a tight bandage, and left on until the following morning, so as to take the shape of the limb accurately. When dry it was painted over with water glass to prevent chipping. This splint, when tightly applied, made a very efficient and comfortable support, which took the weight off the fracture while walking.

A skiagram at the end of seven weeks—that is, twelve weeks after the accident—showed the malleoli as mere shells of bone (the atrophy of disuse), but at the same time a good deal more bony callus was seen at the site of fracture, which was otherwise unchanged.

At the beginning of the ninth week the patient returned to the country and was able to resume his occupation to a great extent. When out of doors he used two sticks, and the boot of the injured leg was fitted with side steels extending from the heel to the tuberosities of the tibia, where they were attached to a metal collar, which was

tightened by a strap in front. A leather gaiter extended from just above the ankle to above the collar. This apparatus was intended to act like the walking plaster, and though it did not give as much support to the fracture, it had the advantage of allowing restricted movement at the ankle.

I saw the patient last on April 15th, eleven weeks after the operation, on his return from London, where he was transacting business. He was then able to walk fairly well in the boot without the support of sticks. It caused him no pain. Indeed, since the operation, he has never suffered any pain at the seat of fracture, and very little anywhere else. His ankle is still somewhat weak, and, more particularly on this account, I advised him to wear the boot for another month when out of doors.

In conclusion, the case was a typical one for operative interference. Left alone, it would have been months before the bones united, if they ever did so, on account of the interposed muscle—one of the commonest causes of non-union. Besides, the fracture was in imminent danger of becoming compound at one time, and it was only the great care which the patient received in the country that prevented this complication. By exposing the fracture as described, it is possible to obtain a perfect view and access to the site of fracture without disturbing the blood supply of the periosteum, or injuring the soft parts. One can see at a glance if the bones are in proper alignment, and that there is no rotation of the fragments, an important point in considering the amount of pain and disability the patient will suffer when he comes to use the limb.

Mr. Hayes said he had never seen better X-ray pictures. It seemed to him extraordinary that, having regard to the fact that

the fibula was not broken except high up, and displacement was so little, more union was not attained. Of course the intervening muscle may have affected it. An X-ray photograph should be taken in every case where there was a suspicion of fracture, as fracture had been thus found in cases where there was no indication of it.

Mr. Harvey said he had a case of compound comminuted fracture in which he had operated ten days after the injury, and there was not the least trace of callus. He had used a plate instead of wiring, and he had noticed the other day, about three weeks after the operation, that the upper end of the lower fragment was more or less necrosed.

Mr. Gunn said that the case before them was a typical one for wiring. He was not quite clear that the X-ray photograph was an unmitigated benefit if taken after the fracture was set. In some cases the amount of displacement might appear to be greater than it was in reality. He had found the bones in better apposition than the photograph showed.

Mr. Blayney said it had been pointed out that the greatest difficulty of reduction had been experienced in cases of spiral fracture; this was probably due to the entangling of the ends of the fibula.

Mr. Lentaigne said the French method of treating fractures by massage was good enough where there was no deformity; but where there was much deformity, and no apparent attempt at union, undoubtedly something had to be done like what Mr. Stevenson had carried out. He had been rather afraid that some harmful result would come in cases where he had used wiring circularly round the bone, but no harm had ever followed. He had recently treated a girl's tibia by wire and plate. Suppuration followed, but he found she had a habit of running a hairpin down under the dressing when the surface itched. He cut down and removed the wire, and the girl walked about at once. Even where suppuration did take place, they might have perfect union, and no necrosis, and very little trouble in removing the foreign body.

Mr. Stevenson, in reply, said he thought the callus ought to show in about a fortnight, or a little sooner, in an X-ray photograph.

ABSTRACTS.

SECTION OF SURGERY.

Friday, November 19, 1909.

The President—Mr. J. Lentaigne, in the Chair.

The President said that last year in opening the session of the Section he had spoken of the "present position and future prospects of the Academy," and had then been compelled to draw a rather gloomy and depressing picture. He had had to deplore the indolence and apathy which, in his opinion, were steadily destroying the life of the Academy. He thought he could not do better, in opening the present session, than return to the subject. Looking back over the work done during the past session, he was sorry he could see no evidence of improvement in respect of attendance. The number of total attendances at the six meetings of the Section had been 152, or an average of about twenty-five at each meeting, as compared with 167, or an average of about twenty-eight the previous session. An analysis of the attendances showed that the Fellows had been a little more attentive in the past session, but it was a curious thing that no attendances were recorded by students, as against 21 in the previous year. He was, however, glad to say that the record of work done was a much more cheerful one. During the past session they had had many valuable papers, and many cases of extreme interest had been exhibited, which made him all the more surprised that the attendances had been so small. He considered that the record of papers and exhibits brought forward was one of which any Society like theirs might well be proud, but small, in view of the fact that there were over seventy men in Dublin engaged in the teaching and practice of surgery, as well as hundreds of students to whom the meetings of the Section should be of great importance. He asked those present to assist in improving this state of affairs.

On the Technique of Operations for Fractures of the Femur.

Mr. R. Lane Joynt read a paper on the above. See page 107, *ante*.

Some Difficult Cases of Fracture requiring Operative Treatment.

Mr. W. I. de C. Wheeler read a paper on the above. See page 127, *ante*.

Friday, December 17, 1909.

The President in the Chair.

Some Points in the Operative Treatment of Malignant Disease affecting the Large Intestine.

Mr. Edward H. Taylor read a paper on the above. See page 137, *ante*.

Empyema of the Ethmoidal Cells with Symptoms of Trigeminal Neuralgia.

Mr. O. Gogarty read a paper on a case of suppuration of the ethmoidal cells, with symptoms of trigeminal neuralgia. A man, aged thirty-one, had been suffering for thirteen years from left side facial neuralgia of epileptiform type. The attacks were frequent, and often recurred several times in an hour. For nine months he lived on fluids alone, being unable to masticate. He described sores on the palate. In 1902, all the teeth, though sound, were removed above and below from the left side. Later the infra-orbital nerve was resected. He would not submit to removal of the Gasserian ganglion. In 1907 he was sent to me by Dr. Moorhead, Royal City of Dublin Hospital, to exclude possible involvement of the accessory sinuses. Trans-illumination revealed nothing. There was no atypical hypertrophy in the nose. On the antrum being washed out, a streak of pus was found. Both antrum and frontal sinus were sound. It was decided to open the ethmoidal cells. Pus came away freely when this was done. Subsequently necrosed bone and debris were removed. There has been no recurrence of neuralgia since March, 1907.

Mr. Murphy said the importance of early diagnosis and examination in such cases could not be over-estimated. The mechanical difficulties of getting at the ethmoidal sinus were very great, especially in a narrow nose.

Sir Thomas Myles desired more information as to what was meant by disease of the ethmoidal sinus. Was it possible that cases of so-called trigeminal neuralgia which had not been cured, had been due to the presence of the condition dealt with by Mr. Gogarty ?

Mr. E. H. Taylor thought the communication ought to be widely circulated, as it might be the means of bringing before the notice of specialists that in a certain proportion of cases a cure could be brought about by what, in Mr. Gogarty's hands, were apparently simple measures, rather than having to resort to such an extensive operation as the removal of the Gasserian ganglion.

Mr. J. Lentaigne agreed with Mr. Taylor as to the desirability of giving a wide circulation to the paper, which was of the utmost importance and value.

Mr. Gogarty, in reply, said his method of dealing with the case was as follows :—The anterior third of the middle turbinal bone was removed with Grunwald's scissors and Stoerk's snare, and the superior margin of the cut surface punched away to the lamella with a conchotome. Five days later a sharp curved knife (Hajek's hook) was pressed against this, turned into the ethmoidal cells and pulled downwards. Pus at once came into view. A paroxysm ensued. When the patient recovered the opening was enlarged, and the pus followed with a conchotome. The anæsthetic was local—40 per cent. of cocaine solution. No tampon was introduced ; the anterior naris being plugged with cotton wool and the patient warned against blowing his nose.

On the Continent 92 per cent. of cases of empyema in the nose were supposed to come from influenza, and he considered that his case probably began with influenza infecting the frontal sinus, the decomposition in the confined space breaking down the septa and giving rise to the empyema.

Friday, February 11, 1910.

The President in the Chair.

Traumatic Paralysis of the Arm.

Mr. C. A. Ball exhibited a young soldier who, last May twelve-months, was driving a wagon, when the horses ran away. Some

canvas on the wagon fell on him, and he was unable to extricate himself, and became jammed between the horses. He fell on his shoulder, and sustained a fracture of the base of the skull and an injury to the brachial plexus of the left arm. He had recovered completely from the fracture, but there had been no improvement in the paralysis resulting from the injury to the plexus. The lesion had been apparently localised to the fifth and sixth cervical nerves, but the nerve of Bell and the nerve to the rhomboids were perfectly normal, and the lesion must therefore be below the origin of these. He was anxious to know whether he should leave the patient alone or whether there would be any use in making an anastomosis between the sixth and seventh nerves.

Complete Tear of Brachial Plexus.

Mr. Ball exhibited for Mr. E. H. Taylor a patient with complete tear of the brachial plexus. The patient, a young man, aged nineteen, four months previously had become entangled in a threshing machine. A spindle had gone in above his clavicle just over the subclavian artery. He had now complete paralysis of all the muscles of the left arm, complete loss of sensation, and the subclavian artery had evidently become obliterated. It was now four months since the accident, and there was no sign of improvement in sensation.

Traumatic Asphyxia and Injury to Brachial Plexus.

Mr. H. Stokes exhibited a case of traumatic asphyxia and injury to the brachial plexus in a young man whose coat had been caught in a revolving shaft, which wound up his clothes and collar and strangled him. He recovered consciousness twelve hours after the accident, which occurred a fortnight ago, and two days later he began to show ecchymosis of his eyes. His left arm was flaccid; there was a scar on the skin caused by the clothes. He had a certain amount of paralysis of the left arm. He had had complete loss of sensation all up the left arm, but it was now confined to the fingers.

Dr. Moorhead described three similar cases which he had had in a single session three years ago. He did not think there would be any use in doing a nerve anastomosis unless there was some slight reaction to the galvanic current.

Mr. W. S. Haughton said his experience of immediate nerve

suturing had been very satisfactory; but cases in which he had attempted suturing at intervals of five months to two years had not been satisfactory. He had tried grafting a piece of sciatic nerve from a freshly-killed sheep into a man, but the result was not satisfactory.

Mr. W. I. de C. Wheeler said it was difficult to find a good account of such cases in surgical literature, although they were fairly common.

Dr. Pearson said the injury in Mr. Ball's case was of the type referred to during delivery, where pulling the head away from the shoulder stretched and ruptured the nerve trunks. He thought an anastomosis would be better than implanting into the seventh cervical. In implantations, sound nerves found their way to their original distribution, so that in a lateral implantation of the fifth and sixth into the seventh it was possible that the seventh might not supply the others, but keep to its own function.

Mr. Stokes quoted a case of fracture of the clavicle and traumatic aneurysm. The man was operated on and was now alive, but with complete paralysis of the arm. It appeared that the scar of the aneurysm had involved the nerves, and he suggested something similar in Mr. Taylor's case.

Mr. Blayney said he had met a case of injury in which hæmorrhage occurred in the spinal cord, and the patient was left with a considerable amount of paralysis in sensation and motion. He thought Dr. Ball should make an attempt to find the site of the lesion, and suture the two ends of the nerve together.

Mr. Lentaigne said that if he had two such cases as Mr. Ball's and Mr. Taylor's, he would make an attempt to operate, but he would do so without much hope of success. His own experience of late suturing of torn nerves was not satisfactory; the operation was often an exceedingly difficult one. He thought there was no reason to doubt that the last case would probably recover, as the amount of injury was small compared with the other two cases.

Mr. Ball, in reply, said the muscles in his case reacted to the galvanic current, and he would, if allowed, make an attempt to cut down and see what could be found.

Mr. Stokes also replied.

Operation for Fractured Femur in a Child aged Four Years, with Skiagrams.

Mr. W. I. de C. Wheeler showed a case of a child aged four years who sustained an oblique fracture of the femur with marked over-riding of the fragments. Treatment by reduction and immobilisation in plaster for fourteen days failed to completely reduce the shortening. A long incision on the outer side of the thigh exposed the fracture, the sharp ends were imbedded in the muscles, but extension had little effect in bringing the fractured extremities into apposition. After some difficulty the fragments were replaced and held together by the application of two Lane's plates and six screws. Three encircling aluminium bronze wires passing through holes in the steel plates and round the oblique ends of the fracture completed the operation. Recovery was uninterrupted and with no shortening of the injured limb. He strongly advocated operation in fractures of the femur in cases where by X-rays it was evident that anything like good apposition of the fragments was impossible by non-operative methods. Pain and discomfort disappear after the operation, and little or no subsequent treatment is necessary. In a child no splint need be used after the first few days. In about six to eight weeks the patient is allowed to use the limb, the joints being freely movable by that time. Skiagrams were shown to illustrate the condition before and after operation.

Dr. Harvey said it was impossible by the old methods to get the bones in proper position by extension and counter-extension.

Dr. Stevenson said that an X-ray photograph should be taken through the plaster, as the removal of the plaster might bring about the shortening again.

Mr. Stokes recalled two cases in which he had had to remove wire after some years.

Mr. Haughton said that in cases of oblique fracture, when the attempt at reduction by extension and fixation was thoroughly tried, and the result failed to satisfy the X-rays, an operation was indicated to replace the bones truly.

Mr. Lentaigne said he had himself taken out a good deal of wire, and also screws and plates, but he would not consider

that the slight disadvantage of removal counterbalanced the advantage gained by operation.

Mr. Wheeler, in reply, said the trouble experienced after prolonged plaster was very great compared with the trouble of taking out a screw if such became necessary. The great point in favour of operation in suitable fractures was that there was practically no after-treatment.

Multiple Bone Tuberculosis Two Years after Treatment.

Mr. Lentaigne exhibited a case of multiple bone tuberculosis two years after treatment. The case was one of tuberculosis of the wrist, elbow, and face, and of the bones of both feet. Treatment had covered about a year, and had ended two years ago. The patient was completely cured of the tuberculosis, notwithstanding recent statements that such was an impossibility. He excised the wrist, which was swollen to six times the normal thickness. She was scraped in the different tubercular foci, and he amputated two toes. The treatment was simplicity itself, and within the reach of any one. The wrist was left somewhat stiff, but he believed it could be made much freer by massage and by forcible extension and relaxation under an anæsthetic.

Dr. Harvey said he had tried fibrolysin in stricture of the urethra and in cicatrisation after burns, but was not in a position to know the ultimate result after the patients left the hospital.

Mr. Haughton inquired if Mr. Lentaigne had used cautery, or carbolic acid, or other corrosive in the treatment of the soft parts.

Dr. Stevenson asked if tuberculin had been used, or had the scraping been depended on to act in the same way as tuberculin did in increasing the power to overcome the tubercular deposits.

Mr. Lentaigne said he had tried fibrolysin solely in a case, with perfect satisfaction. Another case was still under treatment, and had enormously improved. His method of treatment of tubercular disease was to scrape first, and then to introduce pure phenol in large quantities. In addition, he gave large doses of chloride of calcium. The patient had got two injections of tuberculin.

Fracture of the Humerus.

Mr. W. S. Haughton demonstrated a case of oblique fracture through the tuberosities and surgical neck of the humerus treated

by plates and screws. The patient, a heavy woman, aged fifty, two months previously slipped on ice, fell back, and tried to save herself by putting out her arm backwards. The fracture was consequently due to indirect violence. X-rays showed that all efforts at reduction had failed. As union in the deformed position results in pain, stiffness and loss of power, it is a type of fracture in which modern methods of diagnosis and modern surgical technique will achieve great success. The type of fracture alluded to is one near a joint where we have a small piece of bone (the head of the humerus in this case) which cannot be held or controlled by external means ; while the rest of the bone (the shaft) furnishes a long lever on which the muscles exercise great force and cause permanent deformity. The older methods fail to produce good results in this class of fracture, and it is in such cases that true mechanical fixation by steel plates screwed in under precise aseptic conditions promises the most brilliant results. This case was so treated as shown in the skiagrams for which Mr. Haughton was indebted to his colleague, Mr. Stevenson. An incision was made along the anterior border of the deltoid, a few of the clavicular fibres were divided, and two plates screwed in. Passive motion of the joint was started five days afterwards, and continued daily since.

Mr. Lentaigne said the result was most satisfactory and encouraging. The treatment was very young, and was capable of much development.

Mr. Haughton, in reply, said he approached such cases with an open mind, and acted as the circumstances required. In the neighbourhood of the joints, Y-shaped plates often got one out of difficulty, but he usually used wire with satisfactory results.

Friday, March 11, 1910.

Mr. Henry Stokes in the Chair.

An Unusual Fracture of the Tibia and Fibula treated by Screw-plating.

Mr. W. S. Haughton read a paper on the above. See page 160, *ante*.

A Case of Thrombosis of the Lateral Sinus with Resection of the Jugular Vein.

Mr. O. Gogarty read a paper on the above. See page 169, *ante*.

Friday, April 22, 1910.

The President in the Chair.

Two Cases of Nephrectomy.

Dr. R. Atkinson Stoney read an account of two recent cases of nephrectomy. See page 177, *ante*,

Coxa Valga, with Report of a Case associated with Ankylosis of both Hips.

Mr. Seton Pringle read a paper on above. See page 185, *ante*.

Un-United Fracture of the Tibia treated by Wiring.

Mr. Walter C. Stevenson reported a case of above. See page 193, *ante*.

SECTION OF OBSTETRICS.

THE TEACHING OF PRACTICAL MIDWIFERY IN THE PAST AND AT THE PRESENT TIME.

By HENRY JELLETT, M.D., F.R.C.P.I.;
King's Professor of Midwifery, School of Physic, Trinity College, Dublin.

[Read in the Section of Obstetrics, November 5, 1909.]

The subject which I have selected as the basis of my Introductory Address is one which has been prominently before the various bodies interested in medical education during the past couple of years, and which has also been the subject of an important debate in this Section of the Academy. I propose to refer to the official regulations which have existed for the teaching of practical midwifery in the past and in the future. The members of this Section labour under no delusions as to the importance of the subject. They see daily, both in their hospital and in their private practice, the results of its neglect in the past, and many of them have worked hard to effect improvement. It is only too obvious, however, that the bulk of what may be called the governing bodies of the medical profession have taken a long time to appreciate the needs of, and for, a thorough practical education in Midwifery and Gynæcology, and though that lack of appreciation was more marked in the past—as I think my address will show—it is still existent. So far as my own experience goes it seems to me that gynæcologists and obstetricians are able to take a broader and saner view

of the relative importance of subjects in the medical curriculum with which they are not personally concerned than is the physician or the surgeon. It is only a short time ago that, during a discussion on the teaching of midwifery, a prominent medical man decried the necessity for improving it, and pointed out that already more time was devoted to the teaching of it than to the teaching of diseases of the eye, while, said he, all humanity have two eyes, whereas only a part of it has one uterus. Nor was he willing to accept the fact that the general practitioner—for one eye case which he meets which cannot wait for the services of a specialist—meets, I suppose, twenty cases of labour whose safety and future welfare are entirely in his hands.

It may be said that this is only the special pleading of a specialist for his own specialty. But what specialist has ever yet advocated the extension of obstetrical and gynæcological teaching beyond the degree required by the general practitioner? All our complaints have been directed against the fact that, while it is the object of all medical education to turn out not specialists but general practitioners, education in one of the most important branches of general practice was neglected and almost reduced to a farce. It is not so long since the General Medical Council considered it sufficient education in practical obstetrics if a student attended twelve cases of labour, of which he was to conduct three personally under the supervision of any registered medical practitioner. When efforts at reform were made, the upholders of such a system openly expressed the view that the student, as regards midwifery, must obtain experience after qualifying "in the same manner as he does in respect of medicine and surgery." If the subject of midwifery was comparable in its extent with a single disease, such, for in-

stance, as typhoid fever, then the argument might have some weight, since students must perforce go out with only a scanty practical knowledge of individual diseases and acquire a fuller knowledge later. But can such a comparison be made? Is it not more correct to compare midwifery with a group of diseases, such, for example, as the diseases of the thorax, and what clinical teacher would suggest that it was sufficient for a student to examine three cases of thoracic disease in order to acquire a general practitioner's necessary knowledge?

In the spring of 1888 the General Medical Council received a report from their Education Committee, and one part of this dealt with "the importance of increased attention to general and special clinical work." We are concerned with only one of the several recommendations brought forward by the Committee, and this was to the following effect:—

"That every student should be required either to attend for three months the indoor practice of a lying-in hospital, or to have been present at not less than twelve labours, at least three of which he should have conducted personally under the direct supervision of a registered practitioner."

An amendment to the effect that the number of labours to be attended by the student should be increased from twelve to twenty was rejected, and the recommendation of the Committee was adopted, thus rendering it possible for a student to be qualified to practise midwifery after he had personally conducted three cases of labour under the supervision of any medical practitioner.

In 1891 the teaching of practical midwifery again came before the General Medical Council as a result of a motion moved by Dr. Glover, and seconded by Dr. Atthill, to the following effect:—

"That it is urgently necessary for the Examining

Bodies to require of candidates for their diplomas additional guarantees of their practical education in obstetrics; and that candidates at the final examination be required to produce proofs that they have personally conducted at least six, and have been present at at least twenty-four additional labours, or of having attended as pupils for six months a lying-in hospital, or the lying-in department of a general hospital, or a maternity institution, with arrangements for clinical teaching, approved by the Examining Body granting the diploma.

This very moderate proposal was defeated by an amendment which whittled it down a little, only to be itself defeated by another which still further reduced it. Finally, the opponents of any reform considered they were sufficiently strong to move that the Council resolve to adhere to its existing resolution, and this was carried, and the *status quo* maintained.

In the winter session of 1895 the General Medical Council received a petition from the Council of the British Medical Association to the effect that the members of the Association viewed with deep concern and regret the admission of students to their final examination after they had personally conducted only three and been present at only nine confinements. The Council of the Association, therefore, petitioned the General Medical Council to recommend that no student be admitted to his final examination until he proves that he has personally conducted at least thirty confinements under the direct supervision of a medical practitioner. This proposal removed some of the farcical elements of the existing state of affairs, but inasmuch as it recognised any general medical practitioner as a suitable person to teach practical obstetrics, it would have failed in its object. The petition was received by the General Medical Council, and

referred to the Education Committee for consideration and report, a step which was at least a recognition that the existing state of affairs was not perfect, and so was a great advance on the action of the Council in 1891.

The Education Committee, so far as one can judge from the report presented to the Council, gave the matter a very careful consideration, and, though it is impossible to recognise their resultant recommendation as anything but insufficient, it is evident that they had to contend against very serious opposition to any step for the improvement of midwifery teaching. The inevitable compromise resulted, and, while students were to be allowed to put in a three months' attendance at a midwifery hospital if they wished, they were also to be admitted to examination after being present at not less than twenty labours, at least five of which were to be personally conducted under the direct supervision of a registered medical practitioner. This recommendation was adopted by the Council, and remained as their recommendation to the licensing bodies until the adoption, in 1907, of the series of resolutions to which I shall now refer, and which signalled the advent of an entirely new system in the teaching of practical midwifery.

At the May meeting of the Council in 1905, on the proposal of Sir John Williams, it was resolved to appoint a Committee of the Council to inquire into the operation of the rules regarding the midwifery practice required of candidates for medical qualifications. This reference covered a wider field than any previous reference made to any Committee appointed to inquire into the teaching of practical midwifery. The Committee, which had reported in 1895, had been limited in its scope to inquiry into the attendance by the student on cases of labour under the supervision of a medical practitioner. The

reference to the present Committee covered the whole teaching of practical midwifery.

With the object of obtaining information on the subject a series of questions were sent by the Committee to "the licensing bodies, which include teaching institutions, and to other teaching institutions which are recognised by the licensing bodies for subjects other than those of the first year's course." In this respect I think that so far as Ireland, at any rate, was concerned a mistake was made, inasmuch as many of the questions asked came within the purview of the teaching midwifery hospitals rather than of the licensing bodies, who were unable to answer several important questions. It is impossible here to enter into the different questions asked, but one question and its answers may be mentioned:—

Question.—What proportion of your students "attend for three months the indoor practice of a lying-in hospital" as an alternative to not less than twenty cases of labour?

Summary of the answers received from thirty-seven institutions.—In eleven schools none of the students attends for three months the indoor practice of a lying-in hospital "as an alternative to twenty cases of labour"; and of these in ten others practically none does so. About 50 per cent. of the students in one school take the alternative. Almost all the students in six schools take the three months' hospital course.

So far as I can judge from a perusal of the replies received by the Committee the taking of a hospital course was a matter of the greatest rarity in any, but in the six schools last mentioned, and in almost all the others, students acquired their knowledge of obstetrics by being present in fact, or in theory, at twenty cases of labour, of which they personally conducted five under the super-

vision of a general practitioner. Further, it appears, and the Committee in their report emphasise the fact, that in the majority of schools even the minimum requirements of the Council were not complied with. It is no wonder then that the Committee went on to report that "it seems obvious that, even if the course of study recommended were in itself sufficient, since its requirements are not complied with, there is no evidence to show that persons obtaining qualifications possess the required knowledge and skill for the efficient practice of midwifery."

The recommendations of the Committee were subsequently twice altered before their final adoption, and so I do not quote them in full. They laid down for the first time three important principles which persist in those finally adopted. These were:—

(1) That the student must have received adequate instruction in medicine, surgery, and midwifery before beginning the study of practical midwifery.

(2) That he must have attended the indoor practice of a lying-in hospital or of a general hospital or poor-law infirmary having lying-in wards, before he is permitted to conduct extern cases.

(3) That the certificate showing that he has conducted the necessary number of cases must be signed by a member of the staff of one of the institutions mentioned in (2), thus doing away once and for all with the system of fortuitous instruction by any general practitioner who might or might not be suited to impart it.

This report came in May, 1906, before the General Council, who ordered it to be sent to all the licensing bodies and teaching institutions, inviting criticism thereon. The criticisms received were then considered by the Committee, and, as a result, certain minor changes were made in their recommendations. The consideration

of the Committee's Report by the Council was, however, postponed in order to give various bodies which had not already replied an opportunity of doing so, and, in consequence of these replies, some further alterations were made in the recommendations. Finally, in May, 1907, the General Council adopted, almost unchanged, the recommendations of the Committee, with the result that, on paper at all events, a very definite and practical effort was made radically to improve the teaching of practical obstetrics.

I trust I have not wearied the Section by this dry *précis* of the past history of the teaching of Practical Midwifery. If I have not, I should now like to say a few words on the probable effect of the recommendations of the General Medical Council on the future of such teaching. The recommendations of the Council are only recommendations, and, as such, can be ignored in part or in whole by the licensing bodies. Some idea as to the probable attitude of the latter can be gleaned from their criticisms of the first report of the Practical Midwifery Committee of the General Medical Council, and in these criticisms there is manifest an extraordinary divergence of opinion. The majority of the London Medical Schools consider it quite impracticable to insist on regular attendance upon the indoor practice of a lying-in hospital. The majority of the Scottish licensing bodies, and of the English Universities, and all the Irish bodies, view the proposal with approval. The University of Manchester expresses "unqualified approval of the recommendation that certificates of attendance on labour shall be accepted only from a member of the staff of a public lying-in hospital, or of the maternity charity of a general hospital." The University of Birmingham considers that "general practitioners should not be altogether excluded." The Professor of Midwifery at

Bristol University College considers "that practitioners who have received their education in lying-in hospitals only are not so reliable as those who have been taught in an extern maternity." The Professor of Midwifery at St. Mungo's College, Glasgow, is "of opinion that a student will learn more by attending in the wards of a hospital and seeing cases properly conducted than by attending scores of cases in their own homes."

These are only a few instances taken at random, but they serve to show the differences that exist. It is probable—indeed, almost certain—that the large Universities whose supply of students is constant and independent of minor alterations in the curriculum, will adopt the recommendations of the Council in their entirety, while licensing bodies who are, so to speak, competing against one another for students, will endeavour to present their curriculum in as attractive a form as possible. Similarly the schools which consider the recommendations impracticable will doubtless continue to do so until the licensing bodies to which the schools act as feeders demonstrate their practicability by making them essential.

Here, in Ireland, we are principally concerned with the actions of the Irish licensing bodies, and, so far as I am aware, the Conjoint Board of the Royal Colleges have so far alone modified their regulations. Their alterations are thus of considerable interest as foreshadowing to some extent the possible action of the other bodies.

Before referring to them, it is necessary to state that for many years past in Ireland the regulations for the teaching of practical midwifery have been sufficient, if the student chooses to benefit by them. Instead of its being possible to become qualified after seeing twenty cases of labour and conducting five, the Irish student had

to attend for six months in a maternity hospital, and to conduct some twenty cases. I emphasise this point because there is a danger present that the efforts of the General Medical Council to produce a "levelling" up of midwifery teaching in England and Scotland may result in producing a "levelling" down in Ireland, and, indeed, we shall see that, in the case of the Irish Colleges, already it has been decided to reduce the course in a maternity hospital from six to three months in order to bring it into closer conformity with the recommendations of the Council.

The regulations of the Colleges which have been agreed on after a prolonged discussion, and which will shortly come into force, are as follows:—

(1) Every student before beginning the study of practical midwifery must have attended a course of lectures on midwifery, and must have held the offices of clinical medical clerk and of surgical dresser.

(2) Every student must attend the indoor practice of a lying-in hospital, or the lying-in wards of a general hospital, for a period of three months, to the satisfaction of the medical officer in charge, and, after having received therein practical instruction in the conduct of labour under the personal supervision of a medical officer, must attend twelve cases of labour under official medical supervision.

(3) Every student must attend on at least thirty-two occasions on the regular clinical instruction delivered in the wards of a lying-in hospital, or in the lying-in wards of a general hospital.

(4) All certificates testifying that the student has complied with the above regulations must be signed by a medical member of the staff of a lying-in hospital, or by a medical member of the staff responsible for the management of the maternity wards of a general hospital.

The foregoing regulations are admirable in many respects, but in one particular they are retrograde. It was unwise from every point of view to reduce the course of hospital teaching from six to three months. It was unwise from the point of view of the student's education, because a three months' period is insufficient, and it was unwise from the point of view of his comfort, because it would be easier for him to obtain the necessary number of cases and the necessary hospital attendance in a period of six than in one of three months. On the other hand, it is most important to have obtained a definite pronouncement that the student must attend at the regular clinical instruction delivered in the wards of a hospital.

What the action of the other Irish licensing bodies may be remains to be seen. I trust that it will be in the direction indicated by the regulations of the Royal Colleges, but that there will be no shortening of the periods of hospital attendance. It is for the Irish school to improve and to increase its present standard, and not to let itself forget that what may be improvement in other teaching centres may be retrogression in Ireland.

There is just one other point to which I wish to refer before I finish. The General Medical Council, in their final recommendation to the licensing bodies, added to the recommendation already mentioned another to the effect that it is necessary for the licensing bodies "in the public interest" to establish clinical and practical examinations in midwifery and gynæcology in connection with the qualifying examinations. At first there was considerable uncertainty as to the practicability of such a scheme, but, when it was examined closer, it was found to be quite workable. Trinity College and the Royal Colleges have already adopted it, and, in future, all their students will have to present themselves for such an examination. Its

value, if it is properly carried out, is undeniable, and it remains with the examiners to see that the examination is a reality.

If we contrast the present state of affairs with what existed in 1888, we shall see what a great advance there has been in the teaching of Midwifery, and this Section of the Academy, as a Section, and many of its members individually, have had a great share in this. In 1888 it was impossible for any student who merely followed official requirements to acquire a sufficient knowledge of obstetrics before he was qualified to enable him to undertake the care of the average case of labour. At present, under the new regulations, it is possible for most students to obtain the necessary knowledge. It must be the object of the future to ensure that it is impossible for him to become qualified without such knowledge.

Dr. Kidd said he agreed with the President of the Section that a period of six months was one which allowed the student to obtain the necessary knowledge with greater ease than if it was reduced to three months. He thought an alternative might be made with regard to the period spent as an intern in attending a lying-in hospital. A man living inside the walls of a maternity institution saw everything that was going on, he lived in an obstetric atmosphere. If attending as an extern student two nights in the week, whether he saw a case or not, it was difficult for him to devote his attention to lectures next morning. He thought too little attention was paid to the taking of ample notes of cases either in the house or in the extern department, and it should be the duty of assistant masters or clinical clerks to see that notes were taken and signed by the officer of the institution before credit was allowed for the case. When the question of the clinical examination had first come under his observation, he had hesitated as to the feasibility of its being made practical or practicable; but experience had shown him that the difficulties which he had conjured up were to a great extent imaginary. A practical test was quite feasible as regards palpation, and the

general conditions in the puerperium and questions could be asked to supplement the history that the student gave. He was disappointed in the notetaking.

Dr. Tweedy thought it was a hardship on the student to cut down the course of instruction to three months. But there was one safeguard in the rules that had lately come into force, which had put the new rule ahead of any of the older forms—the rule that a student had to attend thirty-two times within three months. That rule made it impossible for a maternity hospital to give a three months' course on a one month's residence.

Dr. Gibson thought the period, instead of being reduced, should be raised to twelve months. But if a man lived in a hospital for one or two months, he would learn far more than from attending twice a week for half a year, and should be spared further attendance.

Dr. FitzGibbon agreed with Dr. Gibson. Even in six months, of two nights a week, he did not think one would see a quarter of the number of cases which one would see if resident day and night.

Dr Jellett rose to accentuate what Professor Kidd had said about notetaking. The same thing applied to all other branches of the profession, and the result was that, while their students could hold their own as far as practical subjects were concerned, in competition for the services, they were very much behind English and Scottish students in the reports of a clinical case, and the commentary on a hypothetical case which formed part of the Army Medical Examination.

Dr. Jellett, in reply, said he recognised the difficulties that existed, and the necessity for advancing with caution and care. They might all agree that the caution and care had been very well marked in the past, and trust that advance in the future would be characterised by more boldness. It seemed to him that for the qualified man there was no question that one month inside was better than three outside, but he doubted whether the average student could assimilate what he saw, as he could if it was spread over a longer interval. He was therefore more inclined to agree that the time should be extended to a year. Everyone recognised the practical examination as a great step forward, and the marking should, in an entirely new departure, be lenient at first.

PAINLESS LABOUR.

By J. SPENCER SHEILL, F.R.C.P.I.;
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[Read in the Section of Obstetrics, December 3, 1909.]

We are all aware that for countless ages, from Biblical days to the present time, the process of labour has been associated in everybody's mind with agonising pain—in fact, the pain of labour is, and has been, looked upon as the superlative degree of suffering to which the human body is liable.

True, it is a pain that, as a rule, does not pass uncompensated; the joys of motherhood help in no small measure to erase from the mind the excruciating suffering endured before the little one saw the light of day.

True, also, that the degree of suffering differs largely in different individuals, and in the same individual at different times. Again, there is, as we know, a wide variation in the length of the period of suffering; but, generally speaking, long or short, so-called easy or difficult, child-birth and pain go hand in hand.

Like all rules, even this one has its exceptions; all of us have probably seen or conducted one or more painless labours. It is nearly two years ago since I was called to such a case when all was over; and, on cross-questioning the patient, she assured me "that she only *thought* she felt a pain just before she heard the baby cry." This lady was a primipara, and was normal, as far as I could ascertain, in every respect, including the nervous system;

and she was not lacerated in the slightest degree, the puerperium running an uninterrupted course to recovery.

I have seen two other cases somewhat similar, but not quite so absolutely painless, the whole process of parturition occupying between one and two hours apparently, and the patients stating that the pains were but little severer than a mild toothache.

Of course the personal factor enters largely into the case when estimating degrees of pain, but the first-mentioned patient made the fact widely and loudly known when the nurse happened to touch her with the point of a pin when applying the binder. I mention this fact to show that her sense of the appreciation of pain was in no way blunted.

We have seen, therefore, that in rare instances Nature has ordained that the woman shall pass through the ordeal of childbed without any suffering—without even that physiological amount of suffering which we are accustomed to associate with the process—and this without the intervention of human skill. Naturally, this makes the obstetric physician stop and think a little. If the enormous blessing given to a sufferer when surgical interference is called for (as in amputations and such like), in the form of æther and chloroform, could be administered throughout the process of labour to all women, and that without any attendant dangers to mother or child, common humanity alone would be sufficient to prompt us to give it. But the risks, the constant watching necessary, the stoppage of pains, and, therefore, prolongation of the whole process, associated with these forms of anæsthesia, render them unsuitable in the vast majority of cases. Can nothing, then, be done to imitate these cases of physiologically painless labour—to alleviate the hard, grinding pains of the first stage?

The expectant mother of to-day, especially amongst the upper classes, seems to bear with less fortitude than her ancestors the suffering entailed by child-birth; perhaps the pace we now live at, and the many evils of greater civilisation, together with the growing tendency of young women to crush down, at any rate for a time, thoughts of motherhood and home, taking instead to pursuits—whether for pleasure or profit—calculated to unfit them later for easy labour, combine to make the process a more difficult and dangerous one. But, be that as it may, the physician has to face stern facts in the form of hard pains, impatient and anxious patient and relatives, with perhaps his reputation for skill at stake.

It is with regard to the lessening of this suffering that I propose to give my experiences of a form of treatment which is comparatively new, and is even in its infancy, in this country; in fact I am not aware of any account having been published previously to this of the use of scopolamine-morphine in labour in Ireland.

I have been working at somewhat of a disadvantage as may be readily understood when I say that my trial of this treatment—so much lauded abroad—has been made in my private practice during the past twelve months or so. I shall not weary you with notes of the nineteen cases in which I have used it, but rather shall state the important points of use, dosage, and effects—good or bad—noted in those cases; and shall append my own impressions gleaned from regrettably short series of cases, and leave you to form your own opinions of it, and to add these to your own experiences, if any, of the scopolamine-morphine treatment.

I have no sympathy with the man (or woman) who holds the opinion that labour pains are "natural," and, therefore, not to be interfered with. Patients in labour

will cry out bitterly that they are too weak to endure the increasing pains of the latter half of the first stage; chloroform here is contraindicated we know in the large majority of cases. Is there nothing more we can do, then, certain in action, and absolutely safe for mother and child?

Since Krönig's paper was first published there have been sheaves of articles written on the subject, giving the experiences of dozens of intelligent observers testing scopolamine-morphine injections upon thousands of parturient women, as well as upon many patients on whom lesser gynæcological operations have been performed. Of this latter class I have nothing to say, my own short experience being solely with obstetric cases. Many contradictory reports have been published by users of this form of anæsthesia regarding the reliability and safety of the injections, especially amongst those who have used it on animals for experimental purposes.

The pharmacological action of any drug will appear to vary when one takes into account the idiosyncrasies of the animals upon which it is used; the varying qualities of the drug used—and in this respect I may say that scopolamine stands almost unique—and, again, the personal factor must account for some of the differences of opinion.

Scopolamine-morphine may have disadvantages and even dangers of its own, and in this respect is in line with *all* other anæsthetics whether general or local, more especially in the hands of the inexperienced; but accidents will occur occasionally, however careful we may be, and if we are to advance in our knowledge of methods to alleviate human suffering, of necessity the path will be marked—in this as in every other form of progress—by the tombstones of some who fell in the cause.

Some specialists hold that a woman may die from "labour shock"—that is, as the name implies, death due to the pains of labour—without a single demonstrable pathological lesion in any organ of the body. *If* this be so, then, a method of converting labour into a painless phenomenon might actually be the cause of lessening maternal mortality.

Before proceeding to give an account of my experiences, let me first say a little about the chemistry and pharmacology of the drugs used, and over which a certain veil of ignorance seems to hang.

Firstly, there exists great confusion in the minds of some regarding the similarity or otherwise of scopolamine and hyoscine. I have it on the word of an analytical chemist of great experience that there are wide differences in the various hyoscines and scopolamines on the market, and that this is due entirely to the methods used in their manufacture, and not to the source of the alkaloid. As a matter of fact, the chief source of hyoscine is the plant datura alba, but from this plant also are obtained hyoscyamine and atropine.

Now, the roots of scopola carniolica give an alkaloid, a mixture of hyoscine and *atroscine* (not *atropine*), and this combination is known as commercial hyoscine or scopolamine.

But the alkaloid atroscine, when tested, differs from pure hyoscine just as atropine differs from its isomer hyoscyamine. Yet here is what a few authorities on chemistry say about the drugs in question.

I quote Martindale, who states:—"Hyoscine consists principally of scopolamine; and commercial hyoscine hydrobromide consists essentially of scopolamine hydrobromide, which, from a therapeutic point of view, is simply an alteration of name."

Binz says:—"When it was discovered that the hyoscine of commerce consisted chiefly of scopolamine, the name was altered to scopolamine hydrobromide, its formula being $C_{17}H_{21}NO_4$."

The *British Pharmacopœia* gives scopolamine hydrobromide as a synonym for hyoscine.

Lest I weary you with chemistry quotations which have only a scientific and relative bearing upon the subject of this paper, let me give you at once the latest information on the subject which I have been able to gather, and which is not only of scientific interest but has a very practical significance.

The terms hyoscine and scopolamine were first used for two alkaloids obtained from the plants hyoscyamus and scopola respectively.

They were then thought to be different, and entered into commerce named with reference to their source. Later investigations, however, showed that they were *chemically identical*, and this has been recognised by the *British and United States Pharmacopœias*. But subsequent research has shown that the alkaloid, which may be called either hyoscine or scopolamine, exists in three stereoisomeric forms—viz., dextro, lævo, and racemic modifications.

It is believed that the "lævo" form acts twice as strongly as the "racemic" form *on peripheral nerve endings;* and this is the point of importance to us. It also seems probable that some writers use the word scopolamine to denote the "lævo" form, and hyoscine to denote the "racemic" form; this would explain the statement often made that scopolamine is more active than hyoscine.

The lævo form exists in plants, and is liable, as I have stated previously, to be altered in the processes of manu-

facture, and to be converted wholly, or in part, into the less active racemic form; this will account for the variations in the tests; the melting point, the optical rotation, and, what is all important to us, the physiological activity, of different specimens of scopolamine hydrobromide.

In connection with this, I have been in communication with Messrs. Burroughs Wellcome & Co., the makers of the tabloids I have used in my experiments, and I have been informed by their chemist that the tabloids "Hyoscine Hydrobromide" (Hypodermic) contain only the pure "lævo" form, and are uncontaminated with any of the various alkaloids with which it is associated in the crude drug. The names, then, Tabloid Scopolamine or Hyoscine, B. W. & Co., are synonymous terms, denoting the lævo-rotatory alkaloid *only*, and the one best suited to the requirements of the obstetrician.

Morphine is well known for its sedative and narcotic effect and for its depressing influence on the respiratory and circulatory centres when injected hypodermically in moderate doses. Atropine, an antidote to morphia, and so frequently administered in combination with it in small doses, modifies the undesirable effects of the morphia. In the same way scopolamine acts as a respiratory and circulatory stimulant, as well as acting upon peripheral nerve endings, and so counteracts any undesirable action of the morphia. Both drugs are valuable general anæsthetics, but *morphia interferes with labour pains*, whilst *scopolamine does not.*

As to the dosage in labour cases, taking the statements of some nine observers, scopolamine is given in doses averaging from $\frac{1}{200}$ to $\frac{1}{150}$ of a grain (.0003 to .00045 gramme); and morphine in doses of $\frac{1}{8}$ to $\frac{1}{6}$ of a grain (.008 to .01 gramme).

Croom has given $\frac{1}{100}$ of a grain of scopolamine for the initial dose in some 30 or 40 cases. The injections are, if required, repeated in from one and a half to three hours, but the majority of workers do not repeat the morphia, and only give scopolamine in half doses after the first dose. For myself I have, in all my cases, used $\frac{1}{6}$ grain of morphia and not repeated it; and of scopolamine I have in about one half of the cases given $\frac{1}{200}$ of a grain, repeating it in four cases after periods of two to four hours. In the remainder of my cases I have given $\frac{1}{150}$ to $\frac{1}{100}$ grain and not repeated it.

The best time to give the initial dose is when the pains are becoming long and strong, and when they recur, say, about every five minutes. The simplest test of the necessity or otherwise of a second dose is the feelings of the patient herself, and her ability to remember recent incidents of the sick-room or to recognise the hypodermic syringe or other article when shown it.

When the first dose is given the room should be darkened, and the strictest quietness insisted upon; the drugs may have their effect in from half an hour or less up to one and a half hours. The patient may sleep for a couple of hours, the labour all the while progressing uninterruptedly; then she may wake up at each pain, becoming drowsy between them. Should the patient become wide awake before the end of the first stage of labour, a second dose of scopolamine may be given; or, should the patient be of a very highly-strung or nervous temperament, a second dose of morphia also may be administered. I have not given the injections at all in the second stage, because then I prefer the older-fashioned chloroform from which the patient can wake up almost immediately after the stoppage of administration: whereas, if scopolamine-morphine were given, the

patient, and possibly the infant, would remain in a drowsy state for a considerable time, perhaps much to the alarm of the relatives and the inconvenience of the obstetrician.

As to the effects of scopolamine-morphine injection upon the patients, our desire is to produce a painless labour without any ill-effect upon—or danger to—mother or child; how far this ideal has been attained I shall endeavour to point out.

The drugs induce sleep, partial or complete anæsthesia, and loss of memory for some of the events of the labour. This loss of memory was peculiarly transient in three of my series of cases. In one, on the third day of the puerperium, the patient, when speaking to me, appeared to quite suddenly recollect my having said to the nurse during the first stage: "What a naughty girl she was!" I well remember at the time I said this the patient was apparently fast asleep, and remained so for a considerable time; so one requires to be cautious even when patients are apparently asleep.

In another case the patient seemed to have forgotten that she had suffered any pain at all until she recalled, some forty-eight hours later, the pains she endured before the injection.

The third case was similar to the last one, except that the injection was given somewhat later in the labour, and the pains must have been felt severely before and some time after the hypodermic. However, she told her relatives of her painless labour that day; they were somewhat dubious since they heard the cries; but the following day she remembered the pains, and said so; yet from her description of them one would gather that she did not recollect any of them in their full intensity.

It has often occurred to me when seeing nitrous-oxide

gas administered at a dentist's, that the patients *do* suffer pain during the extraction, but forget it entirely. I wonder if this could be so with N_2O and with scopolamine? It is certainly an interesting question.

One of my patients was suffering from "chorea gravidarum," the symptoms of which were much exaggerated by the labour. Here the scopolamine-morphine ($\frac{1}{200} + \frac{1}{6}$ of a grain respectively) gave much relief, but it appeared to slightly prolong the labour. The expulsive efforts of the mother were below par, and forceps were required to assist the head over the perinæum.

Other patients I noticed, less fatigued by a severe first stage as a result of the injection, seemed to bear the second stage pains better than is usual. These patients were without exception multiparæ who had known in previous confinements the tortures of a first stage and accordingly appreciated the relief afforded by the injections.

Forceps were used four times in 19 cases; in all four chloroform was given for the operation, but much less sufficed than is usual when scopolamine-morphine is not given.

We know how extremely difficult it is to estimate the probable length of time any given labour will occupy, consequently I am unable to say with certainty whether the scopolamine-morphine method has any effect upon the duration of labour or not. My experience, however, would lead me to believe that it certainly does not shorten labour, but if anything rather the contrary. A disadvantage of its use is that the patient must remain under constant medical supervision, and this alone would condemn it for general use. A few symptoms disturbing to the patient, and perhaps to the physician also, are flushing of the face, dryness of the throat, and thirst,

with accelerated pulse. This last symptom may remain for some hours as in several of my cases; in one instance it remained—somewhat diminished in degree—for two days, disturbing at first my peace of mind regarding infection. One patient vomited, but I am unable to say whether this not unusual accompaniment of labour was due to the injection or not. Several patients talked somewhat incoherently at times, but there was no approach to wild delirium in any of them.

I should not advise scopolamine-morphine to be given, say, within two hours of delivery, if possible, for several reasons. First, the patients appeared to be either less able or less willing to use their abdominal muscles to bear down. Secondly, on account of, as I have mentioned previously, the drowsiness of the mother remaining when labour is over. Thirdly, because I found on two occasions a slight inclination, more than is normal, for the uterus to relax after all was over. Moreover, the infants run a risk of being born sleepy or even in a state of apnœa or partial asphyxia. This occurred but three times, and all the children were easily resuscitated.

I learn from the Continental observers that there are certain well-marked contraindications to the use of scopolamine-morphine in labour. Heart and pulmonary affections, febrile conditions, general feebleness, or anæmia, uterine inertia, and so on. All my cases were apparently normal save the chorea case. I should much like to try it in eclampsia during labour, as the scopolamine would counteract the depressing effects of morphia alone, and would prevent the morphia from diminishing the uterine contractions.

An undoubtedly pleasant after-effect of the injection is the sound and refreshing sleep most of the patients enjoy when all is over and the room quiet once more.

To ensure accuracy of dosage, one must be cautious that the water used for dissolving the tabloids in has been boiled; not only to sterilise it, but in order to precipitate the bulk of the alkaline salts of lime, potash, &c., contained in ordinary tap water, as these salts have a tendency to precipitate alkaloids, and so reduce the strength of the injection.

In conclusion, I may say that I am of the opinion that the injection of scopolamine-morphine hypodermically for the lessening of labour pains presents many advantages to the accoucheur, but it remains to be demonstrated that these advantages are not outweighed in the majority of cases by certain disadvantages. My short series of cases has not either proved or disproved the case to my satisfaction, and it will be for others, with more suitable and available material to work upon, such as the large maternities can supply, to laud or condemn this treatment, so much practised abroad.

Dr. Tweedy said he had observed that if the vagina was tightly plugged labour progressed almost painlessly, no doubt owing to pressure on the cervical ganglia.

Dr. Freeland said they first commenced to give scopolamine at the Rotunda in September, 1908. They now gave gr. $\frac{1}{120}$ scopolamine, with gr. $\frac{1}{6}$ morphine, and repeated it in gr. $\frac{1}{180}$ doses without the morphia. They had given it in about forty cases. One baby was born slightly asphyxiated, but it breathed after a hot bath. The others were born perfectly normal. None of the women showed any abnormalities after labour, except one who got out of bed when not watched. He thought the use of forceps had been less frequent, and he did not think the treatment prolonged labour. The majority of the patients went asleep for an hour or two after injection, and wakened up with more or less demonstration during a pain, and slept between pains. They had three absolutely painless deliveries.

Dr. Solomons said that amongst other cases which he had

treated by the scopolamine method was one which had occurred a few days previously. In that case he gave scopolamine hydrobromide gr. $\frac{1}{120}$, and morphine sulphate gr. $\frac{1}{6}$ when the os was three-quarters dilated. The patient had been very rowdy, but after the injection she slept, and was surprised when informed of the birth of the baby.

Dr. Jellett said he had given the drug in one case, and had obtained a painless labour. There was some hæmorrhage after delivery, and he found it was from a laceration about the vagina. This bore out Dr. Sheill's remark that women under scopolamine-morphine anæsthesia not appreciating the pain were apt to strain very hard.

Dr. Sheill, in reply, said he thought the plugging of the vagina only substituted one pain for another. The cases mentioned showed the disadvantages of the method, which required constant medical supervision.

SOME HINTS AND SUGGESTIONS IN THE TEACHING OF MIDWIFERY AND GYNÆCOLOGY.

By F. W. KIDD, M.D., Univ. Dubl.;
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[Read in the Section of Obstetrics, January 14, 1910.]

In choosing the title of a paper there is, as our American cousins very aptly express it, often "quite a little difficulty," and my object in writing under the title of "Some Hints and Suggestions in the Teaching of Midwifery and Gynæcology" is that it allows of very considerable freedom in the treatment of the subject, both from the standpoint of the writer and from that of any of the members of the Section who may be desirous of discussing any of the questions raised. In writing under such a heading, I am aware that I may be departing somewhat from the traditions of the Obstetrical Section, yet I think, in the present time, when changes in the curriculum in these important branches of study are being suggested, and the attempt being made to carry some of them into practice, that the subject is one of quite sufficient importance to bring it before this Section of the Royal Academy of Medicine. Although I may necessarily reiterate some of the facts and arguments brought forward by our President in his most excellent address, I hope that you and he will kindly pardon me if I seem guilty of the sin of plagiarism. Having been a teacher and an examiner for over twenty years, even if this fact does not give me a claim to speak with authority, it is, in any case, sufficient guarantee that my experience may be of some advantage in the discussion of such a subject as that which we have now under debate.

The Importance of these Branches.—There is, I regret to say, a growing tendency indulged in by some other branches of our profession to make little of those who adopt the specialty of obstetrics and gynæcology, and with regard to whom I have even known the epithets "Vulvarians" and "Guineacologists" applied. The fact is apparently lost sight of, that upon a sound practical knowledge of obstetrics depends not only the life and well-being of the mother, but also the life of what should be her healthy progeny. A knowledge of the rational methods of infant feeding, where from any cause the mother cannot nurse her own child, as well as the ability to diagnose and treat the diseases of infants—the science of pediatrics—is indispensable to the would-be successful obstetrician. One has only to study the tables of mortality of infants under the age of twelve months to recognise the importance of the subject not only to the profession but to the nation at large. We all admit that "Prevention is better than cure": the knowledge of how to feed and treat an infant that is ailing, and putting that knowledge to a practical issue, are often the means of laying the surest foundation for its development into a healthy and useful citizen. I consider that attending a pregnant woman during parturition is only a relatively small part of the duties of the obstetrician. The care of the pregnant woman—often guarding her against risks that may either be anticipated or frustrated—the diagnosis, treatment, and knowledge of the different ways in which pregnancy may be affected by disease and other morbid conditions, as well as the effect that pregnancy may have on these same diseases and morbid conditions, are all of the greatest importance. The successful treatment of a woman who has had a series of abortions or stillborn

children—thus enabling her to carry her child alive to full term—must be regarded as the saving of a life. The judicious care of the parturient woman during the puerperium may make all the difference between good health and chronic invalidism. Even during lactation many conditions may arise in which the assistance or advice of a well-trained obstetrician or gynæcologist is invaluable to the well-being of the patient.

The fact that so many young men, after they are qualified, *make* an opportunity to spend some time as interns in one or other of our lying-in institutions is proof that there has been something faulty in the efficiency of the training and teaching that they have received, either that they had not been able to gain sufficient practical experience during their student days or that, having had the opportunity, they had neglected to avail themselves of it. The responsibilities that attach to the practice of midwifery and gynæcology are not, in any degree, less than those to be encountered in any other branch of medicine or surgery. Nay, I would contend rather that there are greater emergencies in obstetric practice, and as they have to be dealt with on the moment, there is neither the time nor the opportunity to look up authorities in order to see what *should* be done in any given case. No time to "suck the brains" of some colleague as to the best line of treatment to adopt—the safety of the patient often depends upon the knowledge and resourcefulness of her medical attendant. The dispensary doctor, in the lonely cabin on the hillside, deprived of all the advantages and conveniences of aseptic surroundings, such as are found nowadays in all of our modern institutions, when face to face with some serious complication such as a case of atonic post-partum hæmorrhage, rupture of the uterus, or a case of contracted pelvis, earns

and deserves the sympathy of every member of his profession. Two lives are in the balance—sometimes one, sometimes *both* are lost—while sometimes the life of one is sacrificed in order that the other may be saved. It has been argued by some that academic lectures on midwifery, as well as those on some other subjects, are not necessary. Yet I fail to conceive how any other means can be found of instructing the student in his responsibilities before he attempts to take charge even of the most normal parturient case.

Midwifery is full of surprises, and at any time an apparently favourable case may become one of supreme anxiety by the occurrence of some alarming and unforeseen complication. Speaking from experience, I can unhesitatingly state that the older one grows and the greater the experience one has had, the more one recognises the risks and responsibilities entailed in obstetric practice. Yet how rare it is to find a student who really appreciates these same "risks and responsibilities" at their true value. It is an arduous branch of the profession, requiring a sound physique, rendering its followers liable to be called out at all hours of the day or night—sometimes even all night—a circumstance for which there is often very litle consideration shown by others, especially in regard to the routine of the next day's work. The study of obstetrics requires an all-round knowledge of medicine and surgery. Yet, up to the present time, it was no unusual thing for a student to embark on "taking out his cases" without having been required to produce any proof that he had gained the slightest theoretical knowledge of the subject before doing so. As a lecturer I have always warned students against attempting a practical course before they have attended a course of theoretical lectures. Yet I regret to say that I have

known of many cases amongst the students attending my classes in which this advice was disregarded. How then can one wonder at the numbers of cases of mistaken diagnosis! A student cannot be expected to recognise abnormalities by instinct! Under these circumstances I have known a student to leave his patient and go home to his bed—failing to recognise the fact that he was dealing with a severe case of accidental hæmorrhage—and when other assitance arrived it was too late. Two lives had been sacrificed upon the altar of ignorance! I have known a student send a report to the clinical clerk giving the details of an extern case of abortion, which he was attending, in which he wound up by saying: "On examination I find an S-shaped cartilaginous bone in the vagina"—such was his description of a Smith-Hodge pessary *in situ!* I am very hopeful that such errors may be prevented in the future by the new regulation that has been adopted by the Colleges—viz., "That every student before beginning the study of practical midwifery must have attended a course of lectures on midwifery, and must have held the offices of clinical medical clerk and of surgical dresser." The sooner this regulation comes into force the better. How it will work out in practice remains to be seen. Above all it is absolutely necessary before a student undertakes the responsibility of practical midwifery that he should have sufficient knowledge to enable him to make a fairly accurate diagnosis between what is *normal* and what is *abnormal.* Nature is an excellent obstetrician, and in most normal cases, unaided by any outside efforts, brings about a favourable termination in the most approved manner, while a failure on the part of the attendant to diagnose that it is a normal case may only result in what may be described as "meddlesome midwifery." On the other hand, the

failure to diagnose some serious abnormality in time may seriously prejudice a successful issue or may eventually even cost the patient her life. When a student has diagnosed an abnormality provisions are made that his responsibility can be transferred to the more experienced members of the hospital staff. Incidentally, I might mention a reply given to the present Master of the Coombe Hospital by a nurse when she was asked, "What is normal labour?" Her reply was: "A normal labour is one at which there is no doctor present."

As regards the practical working of the new regulation *re* the necessary qualifications of a student before he embarks on his practical course, what does it mean? It means that our general hospitals must increase the number of their medical clerks and surgical dressers, so that there may be no obstacle placed in the way of any student taking out his practical midwifery, and it means that every student before qualifying must have filled these positions of more or less responsibility during his hospital career. There seems to me, however, to be rather a serious omission in the regulation as at present worded. Although it provides for the necessity of having filled the posts of medical clerk and surgical dresser, I am not aware that any specified time has been mentioned during which these posts must have been filled. Unless some minimum period is introduced for the holding of these posts, the whole regulation may be reduced to a farce. These conditions lead up to a suggestion in the teaching of practical midwifery, which I fear has been neglected in the immediate past—namely, the systematic taking of notes on each case that has been attended, and for which the student hopes to obtain credit. The post of medical clerk or surgical dresser in general hospitals should carry with it the duty of systematic note-taking

on the cases treated, and should be an excellent training ground for note-taking in a special hospital. No system trains the powers of observation and the attention to detail so well as that of accurate note-taking it encourages a spirit of research and it impresses facts upon the memory. I maintain as a teacher, with a view to imparting instruction to a student, that no method is superior to that of examining him on the notes he has taken on some particular case, correcting his errors, supplementing his omissions, and drawing attention to details that may have escaped his observation. Soon habits of inquiry, observation, and accuracy become automatic, and are invaluable assets, not only to the student at his examinations, but afterwards to the busy practitioner on his daily round. The want of accurate and systematic note-taking in our Irish hospitals has resulted in the rejection of many of our candidates in competitive examinations, and this statement is borne out by the evidence of those who have most intimate knowledge of the methods of different boards of examiners, and who, therefore, can speak with the greatest authority on this subject. It is an acknowledged fact that the writing of the history of the précis of cases has been a great stumbling block, and has caused most unfavourable comparisons to be drawn between our students and those trained in England and Scotland. Any system that would result in their better equipment for the struggle of competition should recommend itself to teacher and student alike. My final suggestion on this point is that every student desirous of obtaining credit for conducting a case of midwifery, either in the intern or extern practice of any of our hospitals, should take accurate notes of that case on charts supplied by the hospital, and that these notes should be signed or

initialled by the assistant-master, clinical clerk or other responsible authority deputed by the master of the institution for this duty before credit for the case be allowed.

The substitution of a three months' course of practical midwifery for a six months' course, as recommended by the General Medical Council, is to be deplored, and if adopted must result in our Irish students getting in the future less practical experience than they could have obtained in the past. No student could expect to get twenty conductions in a three months' course if attending the hospital as an extern. Want of material to meet the demand for a greater number of cases may have been a most cogent reason, in the eyes of the General Medical Council, for recommending a minimum of such a limited number of cases—and for shortening the period of practical study—and I am not at all sure that the passing of the Midwives Act has not been an element in conducing to limit the supply of material. The necessity of supplying every nurse, who is a candidate for the diploma of the Central Midwives Board, with so many conductions during her training must of necessity diminish the number of cases hitherto available for the practical training of students, and surely it becomes a matter of importance if the student is to be starved of material in order to supply the demands made in the training of midwives. In some other ways I do not think the regulations laid down by the Central Midwives Board are by any means perfect. Let us take for example the following incident which is reported to have taken place quite recently at a meeting of this Board to consider—under the penal clauses of the Act—charges which were brought against some thirty midwives. The charge against one of these midwives was:—"That she had carefully abstained from making any vaginal examina-

tion; that she had been eight hours at a labour case before discovering an abnormal presentation, when she sent for medical help. The doctor found a transverse presentation, the child impacted, and a hand presenting." Dr. Champneys, who presided at this meeting, cited this case as a "proof that those who advocate external examination only are responsible for the lives of a good many infants." Such a statement, coming as it did from the President of the Central Midwives Board, seems very like "putting back the clock." It has been laid down by those responsible for the teaching in the chief maternity institutions in this country that both midwife and student should rely more on palpation than on vaginal examination, recognising as we do the fact that almost invariably septic infections are heterogenetic, and hence the risk of repeated vaginal examinations. From a dispassionate criticism of the nurse's action in this special case I would come to a very different conclusion to that arrived at by Dr. Champneys. I would unhesitatingly say that the lesson to be learned was "better training in, and more familiarity with, the principles and practice of palpation," instead of more frequent vaginal examinations, and that those who advocate the latter are responsible for the lives and well-being of many mothers. If this midwife, who we suppose was penalised, had been properly instructed in palpation at the institution at which she took out her training, her "sin of omission" would not have been so likely to have occurred. How can we reconcile the advice to make more vaginal examinations with the recent teachings of men such as Whitridge Williams and others? Speaking of vaginal examinations he says:—"A much more serious objection, however, is the danger of puerperal infection. No matter how carefully the obstetrician

may have attempted to disinfect his hands, for it is now generally admitted that absolute hand disinfection cannot always be accomplished, and even granting that the use of rubber gloves overcomes this difficulty, the gloved fingers may still carry up pathogenic micro-organisms from the margin of the vulva into the vagina, and thus infect the infant. It is advisable to limit its employment as much as possible, and in normal cases to do away with it altogether." Again, talking about hand disinfection, he says:—"I have shown that pathogenic organisms are present upon the inner surfaces of the labia and the margins of the hymen in at least 60 per cent. of pregnant women, and that the mere introduction of a sterilised glass speculum two centimetres in diameter, which is no larger than the two fingers employed for examination, carries micro-organisms into the vagina in at least one half of such cases. Moreover, inasmuch as the delicate structure of the parts renders their thorough disinfection out of the question, and as the examining fingers necessarily come into contact with them, it must be admitted that vaginal examinations during labour can never be entirely devoid of danger, and they should, therefore, be avoided so far as is consistent with the welfare of the patient." What guarantee have we that a midwife in making a vaginal examination will under any circumstances adopt even the safeguard of using rubber gloves, or if she does use them that she will make use of such proper precautions as will render their use as aseptic and free from risk as possible?

With reference to the innovation of the "clinical and practical examination" in midwifery and gynæcology, which has been recommended by the General Medical Council, and has been already adopted by the Royal Colleges and by Trinity College, I wish to say a few

words. While recognising that a clinical examination in gynæcology presented no insuperable difficulties, the same facilities did not seem to exist with regard to the midwifery portion of the examination. It was recognised that such an examination could not include anything in the nature of an internal pelvic examination or any obstetrical procedure. Nevertheless, Dr. Tweedy and I, who have had the responsibility of conducting the first clinical examination held by the Colleges in these subjects, have found in practice that a very satisfactory and crucial test of a student's knowledge *can* be obtained —getting him to write a history of the particular case allotted to him, to write down the information that he had acquired by different methods of examination, and to answer verbally any questions asked him by the examiner at the bedside as regards diagnosis, treatment, &c. Every student examined was also liable to be asked questions on any case of infantile disease that was available for purposes of examination. The clinical examination in gynæcology comprised the writing out of the history of one case, and the examination of a second case on the gynæcological chair, which included the making of a pelvic examination, making a diagnosis, writing a note with the result and details of the examination, and answering any questions that might be asked by the examiner as to treatment, &c., with reference to the particular case under examination. In analysing the results of this examination, the want of accuracy, of attention to details, and of method and system, which characterised the "written histories" of the patients, very forcibly brought home the necessity of improving the methods of instruction at present in vogue at our lying-in institutions.

In the teaching of gynæcology it has been my practice

to endeavour to inculcate method in the form of the interrogation of each patient. My plan is to try to train each student to ask all those questions which should enable him to elicit a clear history of the conditions which have led to the necessity of the patient seeking for medical advice. Each student is given a case in turn; when he goes wrong his mistakes are corrected, while in suitable cases he is given the opportunity of making a pelvic examination. Here again his errors in the method of examining the patient are corrected—particular attention is paid to questions of differential diagnosis, examination by inspection, palpation, and the bi-manual method—questions of treatment are discussed, and in minor cases he may be allowed to carry out the treatment under supervision. With some teachers there seems to me to be a tendency to teach over the heads, so to speak, of an ordinary class, forgetting that not all the members of the class expect to be gynæcological specialists. What the majority want, and what will pay them best in after life, is a good grounding in the diagnosis and treatment of everyday diseases and ailments, and time spent in making oneself familiar with these latter conditions is, to my mind—as regards the general run of medical student—time far better and more profitably spent than looking on at some extremely difficult and intricate gynæcological operation which no one but a specialist would ever be called upon to attempt. As regards the teaching of midwifery in an academic course of lectures—as it is quite impossible to cover all the ground in a limited course of fifty lectures—it has always been my aim to endeavour to put the student in possession of a good foundation of fact and theory on which he may safely build the fabric of practical midwifery. I think that a lying-in institution is a more suitable place for

teaching by demonstration (such as those upon the phantom, &c.) than in the theatre of an academic lecture.

The post-graduate teaching of midwifery and gynæcology undoubtedly has its uses. Yet to my mind there is sometimes the danger that the teacher, demonstrator, or operator may be led astray from what was the original intention of these courses. It was originally intended that they should provide practical demonstration and instruction on such subjects and cases as would prove of value to those attending the course in their everyday practice. I have noticed, on more than one occasion, a tendency to reserve rare and difficult operation cases for demonstration or operation at a post-graduate class. Teachers lose sight of the fact that such cases are extremely unlikely to be of any practical value to the ordinary run of the members of the profession attending the course, and so they not only lose the opportunity of demonstrating and of giving useful information on cases that would be likely to occur more frequently in general practice, but they may possibly lay themselves open to the suspicion that their main object is to impress the class with a glorified idea of their own skill as operators or demonstrators, either with the possible chance of a consultation in the future, or of having patients sent to them for treatment when suffering in a similar manner.

To the senior members of this Section I owe an apology, if I have introduced subjects with which they are already well acquainted. My only excuse is that these subjects may be perhaps not equally well understood by some of our junior members. I would ask the student portion of my audience kindly to remember that in making any new suggestions, especially with regard to note-taking in hospital, I have not done so with the object of making their task any more difficult, but solely with the object

that they might obtain fuller value from their opportunities, and so be fitted to compete successfully for valuable appointments with others who have at present the advantage of being trained under more favourable methods and auspices.

Dr. Stritch said he had taken out his midwifery in Edinburgh, and had to pass an examination on the course of lectures before attending labours.

Prof. Alfred Smith said he had recently taken up an American journal which, in indicating the correct course for a student to learn midwifery, did not indicate the method of Vienna or Berlin, but that of Dublin. They should try to realise the importance which was attached to anything that went forth from the Dublin School, and, therefore, strive to keep up their standard. The great claim of the Dublin School was that it was essentially practical. The main trend of midwifery examination was to educate the sense of touch so that they could realise the factors present in a case. They had in Dublin ample opportunities in the big maternities for such education. He was an advocate of the three months' system, but it must be a full three months. He believed in a clinical examination in gynæcology, but he did not think they would get much further forward by a clinical examination in midwifery. Gynæcology and midwifery should not be taught together, but in separate three months' courses.

Dr. Purefoy disagreed with Dr. Kidd's inference from the fact that so many students gladly availed themselves of the post-graduate course. He (Dr. Purefoy) took it that their interest had been aroused to such an extent that they wished to carry their studies further. The practice of note-taking had not been developed as it should be. The accurate description of a case which would give a realistic picture of the main features of a case to a person who had not seen it was very often a matter of considerable difficulty. He thought the change to three months was most unfortunate, as it might lead students to estimate the importance of the subject by the time given to the study, and would make it practically impossible to provide the necessary material for enabling students to qualify for a certificate. He

was strongly of opinion that the clinical examination in Midwifery ought to be carried on, as it had only been begun, and would be improved. The more their students practised palpation and educated the sense of touch, the more advantage they would derive in other departments of their profession.

Dr. Dempsey considered that note-taking in a certain number of cases should be made obligatory. Even without its being compulsory, students could be got to take notes much better if the physicians in charge would show more attention to the notes that had been taken. He thought that the fact that classes would be smaller in a three months' course, and that students would, therefore, have greater opportunities for becoming acquainted with the work, was a point in favour of the short course, and he thought that the diligent student would have every facility in Ireland in getting out his cases. The manner of teaching gynæcology was a difficulty. He thought it was best taught in the extern department, but if the extern teaching was to be successful, the number of students must be limited, as patients did not care to have a crowd of men around them when undergoing examination. In Dublin they had a great deal of material for clinical examination, but in smaller centres it might be difficult to get cases that the students would not have had the opportunity of knowing something about.

Dr. Gibson said he had had charts printed which were to be given in the Coombe Hospital to each man going to extern cases. If a man wished to attend a hospital for six months he ought to have a chance of doing so, but men ought to be impressed with the benefit of living in a hospital for a time. This was, in his opinion, the only way to learn practical midwifery, and he considered that a term of residence for at least one month, with a definite amount of work during that time, should be made compulsory. Men should do a good deal of practical gynæcology while doing their midwifery, if they had not already done it. The fact that nurses training for the Central Midwives Board required twenty conductions, and that a student was not allowed to interfere in any way with these conductions, made it difficult to train many nurses for the Central Midwives Board.

Dr. Solomons, speaking as a grinder, thought that it would be fatal to change to three months, as men would not be able to get sufficient practice in the time. Speaking of gynæcology, he said

that if a man attended the Rotunda regularly he would easily get enough practical work to allow him to be able to recognise any normal or abnormal condition. He quite agreed that men should only be allowed to take out their practical course after having taken lectures. For the teaching of night students, lectures were given each night at 11 o'clock.

Dr. Pearson said that students were divided into two classes: the men who were conscientious, and who, if there were no lectures, would make it their business to learn something from text-books, and the men who simply wanted to get through the course by attending the requisite number of lectures on the chance of making a living. If, therefore, lectures were to be of any value, the lecturer himself should give an examination on his own course, and certify that the student is fit to start his practical course. Otherwise they would have the same state of affairs whether there were lectures or not. The value of note-taking would be enormous to a hospital for reference, particularly in extern cases. The notes would also give a student more interest, and make him feel that he was an essential part of the hospital.

Dr. Spencer Sheill said they must not rest on the laurels won by their predecessors. There was a danger, in dwelling on the past, of not advancing with the times. In many things the Americans and Germans were far ahead of them. What had they done in the last few years for the advancement of midwifery, for the ætiology of eclampsia? or what had they done for spinal anæsthesia or pubiotomy? In congratulating Professor Kidd upon his paper, he said he considered it one of the most practical and useful contributions made to the Section for some time.

Dr. Jellett said that Professor Smith and Dr. Sheill served the useful purpose of showing both sides of the question of the position of the Dublin School. There was no question that the reputation of the School was one of the greatest in Europe, but some things in connection with it required stringent treatment. Professor Smith did not really want to alter the period of midwifery and gynæcological training to three months, but to make it two periods of three months each. The suggestion permitted the student to make use of the very large amount of material to be found in general hospitals, and it increased his opportunities of acquiring a practical knowledge of gynæcology. The difficulty

was how to combine general and special hospital work in a single course. They were unanimous as to lectures preceding practice; but if a man who did not satisfy his examiner had to be put back, his whole course would be impeded. He thought that in the teaching of gynæcology, diagnosis should come first—it was the most important thing for a general practitioner with limited opportunities. Next to diagnosis came the indications for treatment, and the teaching of the treatment itself came last of all, as there were few gynæcological conditions that could be treated satisfactorily in general practice.

Dr. Kidd, in reply, said that an explanation of the charge made by Dr. Sheill against the Dublin School might be found in the fact that they regarded the School as being in the van of teaching more in its practical aspect than in scientific investigation.

PREGNANCY AND LABOUR AFTER VENTRO-FIXATION OF THE UTERUS (HYSTEROPEXIS HYPOGASTRIA).

By SIR WILLIAM SINCLAIR, M.D. Aberd.
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[Read in the Section of Obstetrics, on February 25, 1910.]

After reviewing the palliative treatment of uterine displacements with pessaries and colporrhaphy, and quoting Löhlein's statistics showing only 4 to 7 per cent. of successes with pessaries, the reader traced the introduction of surgical operations and their results.

Alexander's operation could be useful only in simple uncomplicated backward displacements, and it has maintained its place as an operation of choice in the treatment of such cases. It fell into a certain discredit owing largely to the lack of judgment on the part of operators who did not recognise its limitations.

Modifications of shortening the round ligaments were gradually introduced which were more formidable than any of the intra-abdominal operations which have ever been practised.

One American operator speaks of his own method as "this bilateral inguinal cœliotomy and shortening of the round ligaments *viâ* the dilated inguinal rings." He declares that it has become very clear that the simple Alexander operation without this inter-peritoneal work is generally too imperfect and imbecile. . . .

Some reference was made to proceedings of Schultze, Schücking, and Sänger merely to call attention to their defects.

In Britain we usually associate the name of Olshausen, of Berlin, with the origin of ventrofixation, but the profession in France claim priority for Kœberlé, of Strassburg. Howard Kelly's account of the origin of the operation is, perhaps, a little ambiguous, but he appears to imply that he performed the ventro-suspension operation before Olshausen published his original paper on "Ventral Operations for Prolapse and Retrodeviation of the Uterus."

Kelly describes in his book his operation of suspension of the uterus. He was much imitated in the United States by operators lacking his judgment and dexterity, and the consequence was a large amount of suffering and danger from operative treatment in labour, including not a few cases in which Cæsarean section had to be resorted to. Kelly's operation gives favourable results in the proportion to which the adhesions of the fundus stretch or give way altogether. The operation, in fact, is only useful if it acts temporarily as a pessary.

The writer claimed for his own operation of ventrofixation or hysteropexis hypogastria that it had stood certain exacting tests and had fulfilled all his hopes and expectations. The tests which such an operation should stand are:—(1) It must be devoid of danger when ordinary care is exercised; (2) it should be easy to perform, and the operator in recommending it must feel certain that he can complete it as planned; (3) it must relieve the symptoms on account of which it was undertaken; (4) it must not produce any new symptoms which will even remind the patient that she has undergone an operation; (5) it must leave the patient free from any remote danger, such as ileus; (6) she must remain permanently free from any sort of physical disability preventing her from performing all the tasks and duties of domestic

and social life; (7) it must stand the strain of pregnancy, parturition, and the puerperium without apparent departure from the norfal; (8) after involution the uterus must maintain the position it was placed in by the operation.

An operation which complies with all these conditions has many positive merits whatever *à priori* objections may be raised against it by preconception and prejudice. The objections usually result from accepting the term "ventrofixation" as expressing a definite procedure, however defective and even irrational may be the *technique* of some operators.

In order to obtain the desired result certain principles must be constantly kept in mind and complied with in every case. It is of the utmost importance that the fundus, uteri and round ligaments are strictly safeguarded, so that if pregnancy occurs it may run a perfectly normal course as if no operation had ever been done. Of no less importance is it that between a sufficient area of the anterior surface of the uterus and the abdominal wall complete and permanent adhesion should be produced. The ideal ventrofixation is that which results from Cæsarean section when the wound is longitudinal, involving the middle third of the uterus, and complete adhesion takes place between the uterine and abdominal wounds. In such cases, when pregnancy again occurs, there are absolutely no abnormal sensations or symptoms, functional or mechanical, owing to the existence of the adhesions. When Cæsarean section has to be performed for the second time it is the height of folly to break down these adhesions.

Some German writers on this subject make much of the need for free movement of the uterus (*Beweglichkeit*). This is a purely *à priori* assumption. There is not a

shred or patch of evidence of the advantages of mobility when the adhesion is on the anterior surface and does not involve the corpus uteri. All the evil reputation of "ventrofixation" has been the natural consequence of dystocia resulting from interference with the fundus, or, worse still, with the posterior surface of the uterus.

The Operation.—The abdominal wound is reduced to the minimum consistent with unembarrassed manipulation, and its lower limb is as far down as possible without interference with the bladder. The uterus is raised up and seized at the isthmus, or just above it, with a suitable volsella, which is held and manipulated by an assistant. If perimetritic adhesions are present they are carefully broken down, and the ovaries and tubes inspected. When the ovaries are found cystic with indurated tunics, they are incised and sometimes partially resected. When the tubes are sealed up, and even when partly distended, they are laid open in some measure and fresh *estia abdominalia* formed.

In the hysteropexis operation itself the first step is to introduce a fine silk suture or two on each side, so as to close the vesico-uterine fold. Then a pair of sutures at each side, well out from the middle line of the uterus, bring the abdominal and uterine peritoneum together. Next, one or two sutures on each side include the fascian peritoneum and layer of uterine margin and return, so that the knot is outside the fascia. The last sutures are transverse, and include fascia, parietal, peritoneum, and a layer of the uterine wall. The uterus must be seized just above the isthmus, best through the puncture wound made by the volsella. During the operation it is easy to estimate the position for the various sutures, so that when they are tied and the uterus is dropped it will sink down to its normal level.

The bowels are kept out of the way, and, if possible, out of sight, by suitable sponges. The abdominal wound is sutured in stages in the usual way.

The proceedings are completed by the introduction of a suitable size of glycerine-pad Hodge-shaped pessary to prevent dragging on the adhesions until healing is complete.

When the operation is resorted to for prolapse it requires to be supplemented by colporrhaphy.

By adopting this method of hysteropexis the uterus is brought as nearly as possible into the normal position of slight anteversion, and the bladder is seldom in any way embarrassed even temporarily.

It is the bringing of the uterus into an almost anatomically correct position without interference with the corpus which is the feature of the operation. Among the important differences between it and the faultier operations is the avoidance of thickening of the anterior wall of the uterus. "My article on 'Hypertrophy and Erosion of the Lip of the Os Uteri, a Pathognomonic Sign of Chronic Flexion,' appears to me to furnish irrefragable proof . . . that in chronic retroflexion the nutrition of the uterine wall varies with the relations of the body and cervix, and in retroflexion there is always thickening of the posterior or concave wall of the uterus. . . . If the uterus is brought into anteflexion to a pathological extent such thickening of the anterior wall and thinning of the posterior wall as to cause dystocia must be the result. . . . All ventrofixation operations in which sutures are introduced into the fundus, uteri or farther back, and especially vaginofixation operations, produce dangerous thinning of the posterior wall during pregnancy."

Many disasters during labour owing to this cause have been reported from all parts of Europe and America.

"Another difference in favour of hysterapexis hypogastria is that it may justly claim to be an effective method for the cure of sterility." No distress during pregnancy or difficulty during labour occurs after this operation. Pregnancy occurring after this operation, instead of being dreaded, should be welcomed as confirming the cure, if for no other reason.

Of the 300 odd patients that had undergone the operation about one-sixth had borne one or more children since. The process of parturition was not distinguishable from ordinary labour, and involution of the uterus went on normally.

Illustrative cases were mentioned concisely.

1. Cure of sterility in patients with retroflexed uterus at time of marriage.

2. Cure of one-child sterility owing to retroflexion of sub-involution uterus after septic endometritis with metritis.

3. Pregnancy occurring after the operation for retroflexion with complications affecting tubes and ovaries, even to the extent of breaking down adhesions, resecting ovaries, and forming fresh *ostia abdominalia*.

4. Labour and use of forceps, normal involution, and normal position of uterus at last examination.

Dr. Jellett said they were under a double debt of gratitude to Sir William Sinclair, first, for the trouble he had taken in coming over, and, second, for declining to avail himself of the privilege of having his paper left undiscussed. He was sure that the paper would lead to a most interesting discussion.

Sir William Smyly said he had come to the conclusion that the very best operation for cases such as had been referred to was ventro-suspension, but through his attendance at the Academy he had been so much impressed with Dr. Jellett's results with the

Alexander Adams' operation that he had recently been trying it. He believed that one should give a woman a chance with a pessary before resorting to operation. His own experience of ventro-suspension had been a very happy one. If he were to read a description of Sir William Sinclair's operation of ventro-fixation simply in a book, he would not think very much of it, but to hear Sir William Sinclair's experience of it put the operation in a very different light. On theoretical grounds it did not seem to him to be physiological to bring the lower uterine segment right up against the abdominal wall; but that did not make the least difference if the results were good.

Professor Alfred Smith agreed that there was a large number of cases in which excellent results were got by the use of a pessary. If every case of displacement was to be treated by ventro-fixation they could not supply the patients with beds in Dublin. The type of case that seemed to him to be most suitable for the Alexander Adams' operation was a simple backward displacement that reduced easily, and was found to have gone back again next day. The aim of operation should be to leave the organ as nearly as possible in its normal position. There was no organ so mobile as the uterus, and he thought its fixation a very great objection. Still they could not overlook the brilliant results in the cases mentioned by Sir William Sinclair. His own practice in cases where he had satisfied himself that the organs were capable of performing their functions was to thrust a forceps through the rectus muscle, and seize the round ligament on either side, and draw them right out through the muscles and suture them there to the aponeurosis. The operation was simple, and the results were excellent; patients returned with the uterus *in situ* without pessary or support. He must, of course, be influenced by the excellent results obtained by Sir William Sinclair from ventro-fixation.

Dr. Horne said he had a patient who had gone through ten confinements who suffered from retroflexion of the uterus, and who had to wear a pessary between the pregnancies. The result of ventro-fixation in 800 to 1,000 cases in America, when the women became pregnant, was that 10 per cent. aborted, and the mortality of full-time pregnancy was 2 per cent. and 3 per cent. In view of such results one felt some hesitation in tying up the uterus. He thought the brilliant results obtained by Sir William

Sinclair were due to the method by which he performed the operation, in suturing above the isthmus, so that dragging on the posterior ligaments did not take place, and abnormalities did not occur when the women became pregnant.

Dr. Tweedy said that Howard Kelly popularised many operations, and gynæcologists owed him and the Johns Hopkins Hospital a considerable debt of gratitude. The operation described by Kelly was the one which he (Dr. Tweedy) personally performed in the vast majority of instances, and he did ten ventro-suspensions to one of all the other methods combined. Excellent results were got by all the methods. He had done ventro-suspension at least 300 times, and in Dublin during the last fifteen years it had probably been performed well over a thousand times. As Master of the Rotunda he saw a number of women, who had been operated on by himself and others, delivered, and he had never once seen a single case that required operative treatment after ventro-suspension. A patient of his had had six normal deliveries after ventro-suspension, forceps or other instrumental delivery was never found necessary, and the uterus was in as good a position as on the first day it was fixed. As he conceived the operation, the uterus was fixed down to the empty bladder, and rose or fell as the bladder filled and emptied. In every instance he had found long adhesions lying directly on to the bladder, so that no intestines could fall beneath them. The adhesions were macroscopically identical with the round ligaments. There was plenty of space for development in pregnancy. He was convinced that after a time, even if these ligaments were severed, the uterus would remain in position, as Kelly's operation was a curative one. Sir William Sinclair's results were brilliant, but Kelly's operation was hard to beat; he supposed its failure did not amount to anything like 5 per cent.

Dr. FitzGibbon said he still believed in the use of the pessary. Women in the child-bearing period were entitled to a period of trial with it. He believed that a large number of cases would be cured of uncomplicated retroversions by wearing a pessary, particularly in those cases where the displacement commenced after child-birth and was probably associated with subinvolution. These cases, if treated with pessary shortly after confinement, frequently ended in six or eight months in complete cure. And a large number of those cases which did not receive treatment

for a year or so after the beginning of the retroversion, if then treated by pessary, and this was followed by pregnancy, would be cured of the retroversion by wearing a pessary for a few months after delivery, and thus allowing the uterus and ligaments to involute fully. In cases of long sterility one was justified in supposing something more than retroversion, and in opening the abdomen to see what was preventing pregnancy. The operation he generally did was Kelly's operation. When a woman was past, or near the end of, the child-bearing period, he did not think it mattered whether they fixed the uterus firmly or not.

Sir Charles Ball said that in the treatment of extensive prolapse of the rectum they almost always resorted to colopexy. Earlier operations had failed largely because a peritoneal surface was stitched to a peritoneal surface. Such adhesions stretched rapidly, and disappeared in a great many cases after a short time. They had therefore to do something more extensive, and to insure that the stitches included the muscular coat of the intestines, and that this was stitched down firmly to the iliac fascia. In the case of a lady on whom he had to do a colopexy, and who had had a retroflexed uterus stitched to the front abdominal wall, he saw no trace of adhesions of any kind, or any mark on the surface of the uterus.

Dr. Spencer Sheill said that, in adding a few words to express his indebtedness to Sir William Sinclair for his excellent paper, in which he had so clearly set forth powerful arguments advocating a most valuable operative procedure, he would like to point out the necessity of approaching any discussion upon the merits or demerits of any particular operation with an open mind. He believed the reason that so many operating gynæcologists were divided in their opinions respecting the advantages of one method of operating for the cure of retro-deviations of the uterus over another method, was because, when a man begins to perform one variety of operation, and becomes expert at it, his cases being uniformly successful, he rarely gives up "the bird in the hand for the two in the bush," and so he views the question with a not impartial mind. He said Alexander Adams is one of the most vigorous opponents of the operations of ventro-suspension and ventro-fixation of the uterus, the results being quite apparent, so it was pleasant to hear from the distinguished guest of the Section the opinion of one who had actual operative ex-

perience has made himself an authority competent to express views upon the subject worthy of closest attention. Dr. Sheill said it appeared to him that there was a place for all the operations—fixation, suspension, and shortening of the round ligaments—and that each had its own special indications. He considered Alexander Adams' operation as useless for the cure of retro-deviations when complicated by prolapse, as ventro-fixation is undesirable in a simple case of backward displacement in a young married woman. As to the respective merits of the external and internal methods of shortening the round ligaments, it was worthy of note, he said, that Adams recommends the former, because he considers the portion of round ligament in the external abdominal ring to be the weakest part, and it being cut away will not permit of recurrence as after the internal operation. It was said that a grave danger of ventro-fixation was "too large an area of adhesion." Dr. Sheill believed the causes of this to be too much injury to the uterine peritoneum, and slight infection of the abdominal sutures, the remedies being obvious. Dr. Tweedy (the Master of the Rotunda) described two thick rope-like ligaments—"in appearance very like normal round ligaments"—as being the ideal result following ventro-fixation; but he (Dr. Sheill) could not agree with him, as he held that there should be a vertical line of adhesion from the base of the anterior cul-de-sac to above the isthmus of the uterus, and not distinct ligaments which were liable to cause strangulation of intestine. He believed the dangers of ventro-fixation in the child-bearing period were dependent upon the placing of sutures too near the fundus in front, or even in the fundus or posterior to it, as has been advocated, as well as upon the adhesion, whether it was peritoneum to peritoneum or peritoneum to fascia. Dr. Sheill said he reported to the Section some years ago a case of this in which he performed ventro-suspension and myomectomy, the patient having previously aborted four times. Again becoming pregnant after the operation, she aborted a twin at two months, carrying to term the other, when she had an uneventful labour. The note of the case was as follows:—"With regard to the operation itself, it is interesting to note that suspension in this case caused no pain, unusual vomiting, or frequency of micturition during gestation, and no dragging or alteration in shape of the uterus other than a slight dimple at the fundus.

Vaginal examination a month later showed the uterus normally involuted and in perfect position." Since then he had delivered her of a second child, and this pregnancy was uneventful in every detail. Dr. Sheill has performed the operation of ventro-fixation on other cases of displacement and descent since then. One patient became pregnant, and labour was normal in every way; in none of the others has there been any recurrence or complication so far.

Dr. Jellett said that he wished to convey to Sir William Sinclair the thanks of the Section for the paper he had read. The fact that it was so fully discussed showed how much the Section appreciated his kindness in coming over to Ireland; he personally thought the use of the pessary a procedure of necessity, but not of choice, and that consequently operation was in most cases preferable. He wished to draw a clear line of distinction between ventral suspension and ventral fixation, as several of the speakers had used the terms as synonymous. Kelly's operation was one of ventral suspension, and it created a bond of union that would give way in pregnancy, or probably without it. In either case it had probably first done its work and enabled the uterus to gain its normal tone. Ventral fixation was an operation intended to create a positive union which would not give way under any circumstances. The absence of trouble during pregnancy after the operation recommended by Sir William Sinclair, which was essentially one of ventral fixation, was due to the fact that he fixed the uterus very low down. Some time ago he (the President) reported some 400 cases of the Alexander Adams' operation from different sources. His statistics showed that there was a return of the displacement in about $\frac{1}{2}$ per cent., while Sir William Sinclair's showed 20 per cent. This showed what an effect the collecting of statistics had on the morality of the compiler. He had personally done some sixty cases, and had only had a recurrence in one case in which he had shortened one of the ligaments only. Kelly's operation, so far as recurrence of displacement or difficulty was concerned, might be put in an almost similar category.

Sir William Sinclair, in reply, said that the remark made by Sir Charles Ball confirmed him in the belief that it was only to the extent to which the adhesions gave way that Kelly's operation was a benefit to the patient. The additional ligaments were not

imbedded, and were not muscular, and would not contract when they relapsed, and to that extent were in the way. He had not discarded the pessary altogether, but statistics over a long series of cases showed that they succeeded in only 11 per cent., and the time the pessary was worn for was varied from a year to seventeen years.

OUR RESPONSIBILITIES IN THE PREVENTION OF INHERITED SYPHILIS; WITH ILLUSTRATIVE CASES.

By J. SPENCER SHEILL, F.R.C.P.I.
Ex-Assistant Master Coombe Lying-in Hospital, Dublin.

[Read in the Section of Obstetrics, April, 1, 1910.]

I have great pleasure in bringing before your notice to-night the notes of two cases, the character of which is indicated by the title of my paper. I take the liberty of again occupying a portion of the Section's time, not because I have anything very new or original to draw attention to, but rather because I consider that these two cases illustrate in a peculiarly forcible manner, not only the risks which the unborn child may run, but also the pitfalls that beset the path of the anxious practitioner.

The first case is that of a lady who consulted me in the month of April, 1907; she was then about eight weeks pregnant, and desired me to produce an abortion for her. Her principal reason for making this request was that she had contracted syphilis some three years previously, and before her marriage, and she was sufficiently well informed to fear the risks of bringing into the world a tainted infant. She had been under medical care on and off since contracting the disease—a throat specialist amongst others—who prescribed for her a suitable preparation of mercury, and advised its continuance for a long period. She confessed to me that she had not persevered with the drug once the secondary symptoms had

disappeared. I warned her of the possibility of spontaneous abortion or of an infected infant, and urged her to take her medicine regularly throughout her pregnancy. I ordered tablets of hydrarg. c. cretâ and opium, and directed the dosage to be pushed to the limit of safety.

This she did to the best of her ability, and I delivered her of a vigorous female infant. Although many authors describe infants—the subjects of congenital syphilis—as being invariably wizened and "old mannish" in appearance, I am of the opinion that this is by no means usually the case. In the present instances, the infants were—as I believe is the rule and not the exception—particularly healthy in appearance when born. In hospital practice some years ago, this was quite a common experience with me.

To resume. On the third day this child developed the usual "snuffles or nasal coryza"; its skin became harsh and dry and of a peculiar dark-earthy colour; the typical coppery rash appeared and rapidly spread all over the head, trunk and limbs. The child lived just two weeks—in spite of all treatment the emaciation was rapidly progressive. The well known wizened appearance developed a week before it died; it took but little nourishment and cried feebly. The buttocks were ulcerated deeply and extensively, and the disease was so fulminating in its nature that inunction and other administration of mercury proved useless.

Now, in this case we have a mother who, having contracted lues some four years previously, and having in a half-hearted way during three years undergone a course of mercury, and then becoming pregnant, having taken a vigorous course of the drug for seven months, brought forth not only a tainted child, but one with enlarged liver and spleen, and so saturated with the virus as to sink in the way I have endeavoured to describe.

Disappointed, but not surprised, at this result—for I feared the ovum would be syphilitic when conception took place—I advised a long course of mercury under supervision, and the avoidance of a second pregnancy for *at least* one year, and, preferably, two or three. This advice the patient took in December, 1907, and she never missed a dose of medicine in the year 1908 (I should say that she refused treatment by intra-muscular injections of mercury). Early in 1909 she again became pregnant, and, taking the medicine without intermission, except for a few days when stomatitis had appeared, gave birth to a fine son towards the end of the year. The child appeared a perfect specimen, but the spirochætes in his system were even more perfect, and multiplied at his expense. The poison was not nearly so virulent as in the case of the first-born, and under suitable and vigorous treatment was soon overcome. But, nevertheless, the child was luetic, and although at the present time he is strong and healthy, yet he labours under a huge disadvantage and runs the risk of further secondary or tertiary symptoms later in life. This case then shows that a syphilitic woman, after two years and four months of vigorous and continuous anti-syphilitic medication, could give birth at the end of that time to a diseased offspring.

The other patient has a somewhat different, but not less tragic, history. She contracted syphilis about ten years ago through an unfortunate accident, the site of inoculation being the mouth. She was under specific treatment for about two years, and according to her medical advisers, marriage was then considered safe. She came under my care for the first time last spring, and her history was as follows:—Nine pregnancies, out of which two were abortions, two miscarriages at four to four and a half months, three at about five months, and

two premature infants at about seven months—one dead-born and the other so feeble that it survived but a few minutes. Examination of the two fœtuses by an expert pathologist was negative. There was no demonstrable abnormality to account for their loss; therefore the patient was pronounced free from syphilitic taint in spite of her past history. The confidence which the patient had in the opinion of her several medical advisers led her to conceal from me all knowledge of the infection, and of any special pathologist's opinion as to the cause of fœtal death; yet her faith did not prevent her from consulting me with regard to her tenth pregnancy. Suspecting lues, I interrogated her husband, with negative result. The patient was now about four months pregnant, was very thin and anæmic, her feet and hands swelled at times, and she was rather short of breath on exertion. The urine was normal in every respect, as also were her heart and lungs, &c.

I should mention that she had undergone two curettages for alleged endometritis. Being lured from the right track by her present symptoms, which appeared sufficient to account for the interrupted pregnancies, and by the absence of venereal history from her husband, I prescribed treatment with a view to enriching the blood, &c., and iron, potassium chlorate, malt, &c., were taken freely. The effects of the drugs were soon noticeable, and steady improvement followed. For the first time she carried to term, and in due course I delivered her of a fine, fat male infant. The child progressed well, and steadily gained in weight, but was unduly cross and irritable without apparent reason.

When four weeks old, the bowels became rather persistently loose, and the nates showed decided scalding. The usual ointment, &c., was prescribed without benefit,

and within a week the scalding gave place to deep and extensive ulceration—unmistakably syphilitic—and nasal coryza also appeared. This time I gave a definite diagnosis to the anxious father, and a confession of the previous history from the mother resulted.

Under anti-syphilitic treatment this child did well; the healing of the buttocks was slow until vapourised calomel was used by Kane's lamp, when the ulcers healed rapidly. It is now doing excellently well, yet although it will undergo a course of treatment designed to remove from its system all traces of hereditary taint, who can definitely say that, sooner or later, some secondary or tertiary symptom may not make itself apparent?

I regret that these two cases which I bring to your notice are by no means rare; I regret it for the sake of the parents and infants, and even for the generation to follow, as well as for my own natural desire only to occupy your time when I have something unusual to bring before you. My excuse to you, then, must be the very reverse; the actual commonness of such cases and their regrettable sadness.

In the diagnosis of congenital syphilis the way is quite easy to any one of experience. Although it is sometimes closely simulated in young children by tuberculosis, yet the family history, the history of miscarriages or still-births, in conjunction with other signs and symptoms, will usually clear up any existing doubt. But there, again, we may easily fall into error, as, although Case No. 1 never had an abortion or any interrupted pregnancy, yet she gave birth to two syphilitic infants. So, even the absence of abortions does not indicate that syphilis is not present, whilst we are all aware that their presence may be due to a dozen other causes.

As in my cases, the wizened appearance is often want-

ing, until other signs have forcibly brought home to us the painful fact of lues. The so-called "syphilitic wig," described by Hutchinson—long, dark, sleek hair—is utterly unreliable; I have seen only *too* many cases of syphilis and have never come across a typical "wig," although I have frequently seen long and dark hair on infants above suspicion.

"Snuffles," more aptly than politely so-named, is of little value as a diagnostic symptom; it is seldom pronounced before the first one or two weeks after birth, because the rhinitis is not sufficiently advanced, and by that time other more reliable signs have cropped up. An enlarged liver and a palpable spleen are, to my mind, of great importance in the diagnosis.

The rash on the third day, coppery in colour, turning to papules, bullæ, dull wrinkled skin, the body wasting away, a hoarse, weakly cry, feeble suckling, and subnormal temperature, combined with enlarged liver and spleen, together make up a clinical picture recognisable at a glance.

The intensity of the infection, in many cases, of congenital syphilis, is remarkable, and finds no parallel in the acquired form; but this is not to be wondered at when one considers that in the former there is no primary stage, unless we assume one to occur during intra-uterine life. The infection is a generalised one when the parent, or parents, were diseased before conception occurred; and it does not appear to alter the case whether the infection took place by way of the germinal elements or, secondarily, by way of the placenta. Holt, in his "Diseases of Children," has stated that "if the parents become infected before the birth of any children, the first-born is almost invariably the one most tainted." This statement is strongly supported by both of my

cases. He says also: "If the mother's symptoms are tertiary, the offspring will probably escape infection." This I cannot now confirm or deny, but No. 2 of my cases had no symptoms of any kind whatsoever, for seven or eight years, other than repeated interruptions of her pregnancies. This interruption may be used as a loophole by Holt, if it is interpreted as an "active secondary symptom," for he says further: "If active treatment of the mother has been employed for several months, if the child is born at a period when no active symptoms are present, the offspring will probably escape." This is when the mother alone is infected.

It may be but a coincidence, but, in the last three cases of this kind I have had under my care, it was the wives who were luetic, and questioning the husbands bore no fruit. It will be a warning to me, if not to others of us, privately to interrogate the wife on the subject, if lues is suspected and a denial of venereal taint is made by the husband. Again, the pathologist's report on the examination of a dead-born fœtus cannot be considered as reliable, unless it include a microscopic investigation. I am unable to say whether, in my second case, the pathologist *did* make this examination or not, but I rather think not, as sections of the liver of a syphilitic fœtus invariably show intercellular cirrhosis, or gummata, and spirochætes are usually early demonstrated when treated by suitable stains. I need not weary you now with recapitulations. The serious responsibilities, the difficulties and the pitfalls are as obvious as the morals to be drawn therefrom.

As to the treatment, the time usually allowed as safe for marriage or pregnancy, following a systematic course of mercury, does not appear in all cases to be sufficiently long. Two or three years may be quite inadequate, as is

only too well shown by the two years and four months' continuous treatment in one of my cases. I know not what period we may advise as being quite safe—not less than three years, I fear, and perhaps even four or more

It is our duty to the individual, to the future generations, and to the State, to take no risks in these cases; therefore let us err, if at all, on the safe side, and prescribe mercury for a prolonged period, and, in order to encourage and assist its regular administration, to order it in convenient form; for inconvenient medicines often mean missed doses, and this, in its turn, means incomplete medication, and, probably, disappointing results.

As to the treatment of the infected infant, I have seen no ointment or powder so effective in healing cutaneous ulcerations as is the vapour of calomel applied by means of Kane's lamp. Mercury, of course, we give orally, or by inunction as well. But which is the better method? If we give hydrarg. c. cretâ in powder form, how are we to know when it has been pushed to the limit of safety? Infants seldom, or never, indicate it by salivation; and if we give it until diarrhœa sets in, it is carried to excess. Moreover, in some children, diarrhœa is induced by very small doses of mercury. Inunction is a dirty method, and may arouse suspicion in quarters where such is to be carefully avoided; whereas hydrarg. c. cretâ is frequently administered in complaints other than lues.

I have a leaning towards inunction for the first few weeks; and, when the infant is fully under its influence, I continue the treatment with hydrarg. c. cretâ orally, adding sodium bicarb. and pulv. cretæ aromaticus if looseness of the bowels results. Iron and cod-liver oil should also be given if cachexia and anæmia complicate the case.

For how long should we continue this treatment? Some authorities tell us to stop when all symptoms have

disappeared, but, lest a relapse should occur, we shall be safer if we continue it in moderate doses for about one year at the least.

For my own guidance I have formulated the following statements:—

1. Having excluded every known cause of interrupted pregnancy—including syphilis—then *treat for syphilis.*

2. Do not place much reliance upon the words of parents, or would-be parents. All men are liars. I speak generically, not of sex.

3. Our previous idea of safe limit of time for marriage or conception in a syphilitic was too short—double it.

4. Our pathologists have, in Levaditi's method and the use of the ultra microscope, a certain method of diagnosticating the presence or absence of *Treponema pallidum* in the liver or spleen of dead suspected infants, if they would only take the trouble to use it.

Dr. FitzGibbon agreed with the statement as regards syphilitic infants being born perfectly healthy. It was still very undecided as to when infected people were free from the infection, as close examination might show that persons who were supposed to be undergoing continuous medication had missed a large number of doses. The old method of giving drugs by the mouth necessitated great trust in the patient, but the present method of injection enabled one to tell whether the patient had kept up the treatment.

Dr. Solomons said there was a baby born in the Rotunda that evening whose mother had had two six-month babies before, and there was a definite history of her having contracted syphilis from the husband. At the beginning of the present pregnancy she came into hospital and started a course of mercury and potassium iodide. She took it regularly in fluid form, and the baby was quite healthy and strong.

Dr. Seymour S[illegible] said they could not lay down a fixed

date, in acquired syphilis, for permitting marriage in both sexes. It was generally taken that man could not infect the ovum after six years, whether he had treatment or not, but woman, he thought, might do it as long as she was capable of childbearing. Ordinarily syphilologists did not like to let the innocent parent know of the condition, as it set up disunion in the family, but they must use discretion. Opinion was divided as to whether a man could infect the ovum at all, as the spirochætæ are supposed to be too large to be carried in the sperm. Some believe that spores, or some resting stage of the spirochæte might be so conveyed, should such conditions be discovered in the life history of the specific organism.

Dr. Pugin Meldon thought the father always infected the mother first without directly infecting the fœtus. The time for the development of congenital syphilis varied very considerably: he had seen a child which was healthy for fifteen years. He did not think the form in which the mercury was taken mattered, but it was important to see that the mercury was absorbed and really passing through the system by periodic examination of the urine.

Dr. Rowlette said it was extremely difficult to say that a child was *free* from syphilis. If one searched for the spirochæte he thought there was no doubt it would be present in great numbers in the liver in cases of congenital syphilis, but the pathologist was not often asked to make such an examination: he was usually asked only to look for gross signs, which might not be at all distinctive. The gross sign on which he relied most was the change that took place in the line between the cartilage and the bone at the end of the long bones, particularly at the lower end of the femur. If one opened the knee-joint and split the bone, one found a very distinct broad white or yellowish band if syphilis were present, inflammatory in origin probably, the cartilage and the bone infiltrating each other. In the healthy child there was a perfectly clean line. But in some cases of congenital syphilis that sign was not present. Generally the liver was enlarged in congenital syphilis; but in some cases it was found shrunken and very small, and with a large and strong fibrous capsule. While the positive diagnosis of syphilis could sometimes be readily made, it was much more

difficult to make a negative diagnosis without definite examination for the spirochæte.

Dr. Purefoy said he had ceased to make inquiries from parents as regards syphilis, as the truth was not likely to be told, and he thought it was better to be guided by the clinical history of the case, and to give mercury guardedly. In such cases it was important to be quite clear that there was no kidney disease. He thought the old-fashioned "snuffles" was generally fairly good evidence of congenital syphilis. A very interesting evidence of congenital syphilis was the form of pemphigus which developed on the palms and soles of the feet of syphilitic infants in the first three months.

Dr. Jellett thought that if one wanted to secure the greatest possible immunity to the ovum one should treat both father and mother. There were a number of conditions that appeared to occur more frequently in syphilitics than in others, such as malformations in which there might or might not be obvious syphilis. Might there not be some disease of the ovum transmitted from the father, some consequence of syphilis, that might be transmitted to the ovum independently of the presence of the spirochæte? It seemed to him that the fœtal liver was always so large in comparison with the size of the infant that a very great increase of size would be needed in order to appreciate the fact that it was morbidly enlarged.

Dr. Sheill, in reply, said he had not noticed any undue proportion between the weight of the fœtus and placenta. He believed it was quite possible to become able to recognise during life in a certain proportion of cases a decided enlargement of the liver in syphilitic infants. He thought it was quite possible for syphilis to be transmitted from the father to the ovum without any demonstrable lesion on the part of the father. Unless the father had an open sore, he did not see how he could first infect the mother, as Dr. Meldon had said. Of course the sperm, if sufficiently large to contain one of the organisms, might first infect the mother presumably through the mucous membrane, and she might infect the ovum through her own system. But if the sperm was able to carry one of the organisms at all, it was much more likely to go directly to the ovum. If mercury was pushed to the point

of salivation, he did not think it was necessary to test the urine. He thought it was a gross injustice to a child if one or other of the parents was not informed of the state of affairs. In later years there might be many effects, and a confession to the doctor might save the child from severe constitutional trouble by wrong treatment. Dr. Solomons had better wait a while to be sure that the child born that evening was really free or not. As regards the administration of drugs, injections were very much better from the therapeutic point of view, but tabloids were easier to take secretly. They should adopt the most efficient form for the particular patient.

Dr. Stritch remarked that the infection might be carried by the seminal fluid.

CLINICAL REPORTS OF THE ROTUNDA HOSPITAL FOR YEAR ENDING OCTOBER 31ST, 1909.

By E. HASTINGS TWEEDY, F.R.C.P.I., Master;
BETHEL A. H. SOLOMONS, M.B., Univ. Dubl.,
AND
J. R. FREELAND, M.D.,
Assistant Masters.

During the year 1908–9 there were 2,369 admissions to the maternity wards and 2,045 deliveries, which is 15 less than the previous year.

The hospital has been improved by the installation of electric light, the planing down of the greatly worn floors, the uniform painting with white enamel of the wood-work of all the convalescent wards, and the modernising of the labour wards by rounding off angles, replastering ceilings and walls, and painting with rippoline enamel.

These labour wards are too small for our present requirements, and will soon have to be reconstructed. The corridors of the hospital and the nurses' home stand badly in need of painting. This deficiency has been commented on in the Government Board of Inspectors' Report, and it is a matter of no surprise to read this comment, for many of our corridors have not been painted for twenty years.

In previous reports I have referred to special points of obstetrical interest which have attracted our attention. This year I desire to refer to eclampsia, contracted pelvis, and the treatment of premature children.

My cases of eclampsia up to date number 73. This is not only the largest series of cases published by one observer

in Ireland, but surpasses the combined number of my two predecessors. It would be of interest to know if the other Dublin maternity hospitals show a like increase. Six of these patients died, a percentage mortality which compares favourably with the results obtained in other clinics (8.22 per cent.).

The close attention given to statistical records of eclampsia make it imperative that a common standard for diagnosis should be established. That this is necessary is evidenced by the fact that some authors classify as eclampsia convulsive seizures unaccompanied by albuminuria, and thus open the way for inaccurate compilation.

The rules we have acted on in classification are as follow :—

(1) All deaths associated with convulsions, occurring after the sixth month of pregnancy, have been considered as due to eclampsia. Before the sixth month the diagnosis rests on the pathological findings. If there is obvious chronic renal disease it should not be classed as a case of eclampsia.

(2) No woman who gives a history of previous fits unconnected with pregnancy or labour should be included, nor has such a case been included in our list.

(3) We have not included any case of convulsions free from albuminuria. This limitation is of the utmost importance, for there are many conditions which simulate eclampsia, such as epilepsy, heat-stroke, &c.

(4) An unconscious pregnant woman, who shows marked toxic symptioms after the sixth month, with albuminuria, is considered to have eclampsia, whether or not she has had convulsions.

It is possible that some cases of true eclampsia may pass unrecorded, but, on the other hand, if a less stringent definition is accepted, it is absolutely certain that many spurious cases will be included in a statistical table.

A close inspection of former reports will demonstrate the fact, which has been repeatedly noted by other observers, that the incidence of eclampsia is apparently in the nature of an epidemic. Thus we have gone months without a case, and then have had four cases in the house at once. This fact is

worthy of more than a passing notice, for it is possible that it may serve as a clue to the recognition of the cause of the disease.

It seems to me that we have five possible explanations to account for its epidemic character :—

(1) The disease is due to a specific micro-organism. This may be dismissed because of its inherent improbability.

(2) Some chance infection by an indifferent organism, such as the influenza bacillus, may determine the onset in a predisposed woman. That eclampsia is the culmination of a high degree of auto-intoxication is now almost universally accepted. I have often thought that certain mild epidemics of influenza or coryza might be its exciting cause in patients predisposed to the attack by toxæmia. The great congestion of the nasal mucous membrane during the attack suggests the disease coryza. I confess, however, that investigations in this direction have led me to no definite results.

(3) Meteorological conditions and seasonal changes affecting a woman with a system saturated with toxins. This is a plausible explanation, but I have never been able to suggest any connection between weather conditions and the actual epidemic. They do not accompany dampness, great heat, great cold, or rapid changes of temperature.

(4) The disease is due to insufficient fluid in the system. In this connection the undoubted fact should be remembered that eclamptics suffer from a thirstiness of the tissues. A great number of pregnant women do not drink sufficient fluid for their wants. It is amongst this class that the toxæmias of pregnancy will be found. It is possible that this lack of fluid may account for the great prevalence of eclampsia in tropical climates, where the natives have schooled themselves to exist on a minimum of fluid. Here again we have attempted to associate seasonal changes with lessened intake of water, but our investigations have led to negative results.

(5) The disease is due to intestinal disturbances. Our final source of inquiry may possibly lead to more definite results. An extensive experience of the disease has convinced us that

intestinal irritation is closely associated with the onset of convulsions. It is rare to see an eclamptic patient with good teeth. A frequent history is that a large meal has been bolted shortly before the attack. In nearly every case efforts to empty the stomach and the bowel have been rewarded by the removal of large quantities of undigested and indigestible material, frequently of a vegetable nature. Thus, potato skins, lumps of apparently raw cabbage, currants and orange pulp have been removed by gastric and rectal lavage. Their removal is usually associated with benefit. Is it not possible that a connection exists between these indigestible masses and the epidemic nature of the disease ? Thus, seasons when raw vegetables are likely to constitute the popular articles of diet, or in the early spring, when the old vegetables have become tough and diseased, would be likely periods for epidemics of eclampsia. The raw meats and vegetables so popular in Continental countries, and the almost exclusive vegetable diet of the tropics, could in this way be made accountable for the greater prevalence of eclampsia there as compared to Ireland.

We think we have gradually evolved a better technique in the treatment of these cases. We have not had a death in our last twenty-six cases. We now insist on greater watchfulness throughout the illness, and seldom permit the nurse to take the entire responsibility. If the breathing becomes impeded and marked signs of obstruction appear, the attendants are instructed to rapidly turn the patient on to her chest, making her head hang over the side of the bed. This allows the fluid which is the cause of the obstruction to pour from the nose and mouth, and free the embarrassed breathing. If necessary this manœuvre is followed by artificial respiration and administration of oxygen. More than one patient has, we think, thus been saved from asphyxia, and one of our recent patients was resuscitated by a night student from what promised to be a fatal attack.

We do not leave as large a quantity of fluid in the stomach as formerly, for we have noticed that it is apt to regurgitate and embarrass the respiration. A full stomach has also a

tendency to press on the weakened heart. To avoid such pressure we keep our patients for the most part on the right side, and only occasionally turn them on to the left side.

In respect to contracted pelves, I would like to enter a plea for their estimation in inches rather than in centimetres. Whatever disadvantage the English method has in other respects it certainly lends itself to a very simple and satisfactory division of pelvic contractions. Thus, if we start with $4\frac{1}{4}$ inches as the normal conjugate, we can divide the different contractions into five degrees, the first four separated from each other by half-inch measurements, the fifth degree comprising all pelves with a true conjugate less than $2\frac{1}{4}$ inches. This presupposes that a simple flat pelvis is being dealt with ; in the presence of general contraction the condition must be considered worse by half an inch.

Children of abnormally large size can be delivered through the normal sized pelvis, and this labour is comparable to unaided delivery in a contracted pelvis. Therefore rules of treatment for contracted pelvis should be based on the supposition that a child certainly of normal size has to be delivered. That this supposition is justifiable is evidenced by our last sixty-one cases of contracted pelvis, with a true conjugate of $3\frac{1}{4}$ inches or less. In these cases the children averaged $7\frac{1}{2}$ lbs. in weight.

I desire to state in an emphatic manner that precision in pelvic measurement is a necessity, in carrying out scientific methods of treatment ; that this precision can be obtained only by Skutsch's pelvimeter, the use of which requires considerable practice ; that treatment based on the known size of the pelvis will yield better results than that obtained by any other single method. Of other methods, the most important is Professor Müller's, which is of immense utility, but not of universal application. It often fails in obese women, or when the child lies in the third or fourth position with the head extended. To carry it out thoroughly an anæsthetic is necessary.

I no longer consider it justifiable in a well-appointed hospital

to induce premature labour or to perform prophylactic version as a treatment for contraction without complication.

These women may be allowed to fall into labour and deliver themselves naturally, provided that the narrowing is not greater than the first or second degree. In such cases the application of a tight binder after rupture of the membranes and the persistent employment of Walcher's position are of immense help.

Delivery by forceps or by enlargement of the pelvis should not be delayed when once maternal or infantile symptoms of distress have appeared, and for these a careful and continuous watch should be kept.

Walcher's position may be maintained indefinitely by placing the patient in the cross-bed position, with her hips elevated by raising the legs of the bed on blocks. This enables the patient's legs to hang down without permitting the feet to touch the floor. Two chairs are placed by the side of the bed, and the patient, covered with blankets, is instructed to let her feet rest on them in the intervals between pains. During the uterine contractions the legs hang down. One of our patients was kept in this position from 11 30 a.m. until 8 30 p.m. without undue discomfort.

In contractions of the third degree the patient's powers should not be wasted by a prolonged second stage, for an enlarging operation will nearly always be necessary for delivery.

In the fourth degree of contraction Cæsarean section, intra- or extra-peritoneal, is the operation of necessity, if the child is alive, whilst craniotomy with cleidotomy, if necessary, can be relied upon for delivery if the child is dead.

In the fifth degree, whether the child is dead or alive, it is best extracted by Cæsarean section.

Of the enlarging operations, sub-cutaneous pubiotomy appears to offer most advantages, both in regard to simplicity and freedom from complication. The one case in which I have performed it will appear in the next Annual Report.

I will again refer to extra-peritoneal Cæsarean section.

To sum up, my views on the treatment of contracted pelvis are :—

(1) Induction of premature labour is never advisable.

(2) Perforation is not permissible unless the child is dead.

(3) Turning should never be employed as a treatment for contracted pelvis, but may still be performed for complications of labour, such as prolapse of the cord, when associated with contractions of the first or second degree.

(4) In the greater degrees of contraction time should not be wasted in an endeavour to obtain natural delivery.

(5) On the other hand, in the lesser degrees ample time should be given the woman to enable her to deliver herself, if possible. Eight or ten hours may be necessary for the moulding of the head, and interference should not be considered until there are evidences of fœtal or maternal distress. Once fœtal symptoms of distress are manifested there should be no delay in delivery. Walcher's position should not be forgotten as an aid to fixation of the head.

(6) High forceps should never be applied until all arrangements are perfected for an operation to enlarge the pelvis. It is, in my opinion, a pity to proceed to the latter expedient until forceps have been tried tentatively. It must be confessed they occasionally accomplish their purpose under the most unexpected circumstances.

Finally, in these, as in all other obstetrical operations, the best results cannot possibly be obtained if rubber gloves are not worn.

The treatment of premature children is, to say the least, disappointing. I have over and over again remarked that these infants do apparently well for about a week. They then become very withered, with a dry, parched skin, and gradually die from asthenia. The change is sudden in its onset, and seems to be closely associated with dryness of the tissues due to lack of fluid. This is not surprising when we remember the hot atmosphere in which the child is kept, the constant loss of fluid through the skin, lungs, kidneys and bowel, and the extraordinarily deficient means at our disposal to make good

this waste. Thus it is often impossible to give such an infant more than a teaspoonful of fluid at a time, and our resources are taxed to the utmost to compensate for the loss. I have tried subcutaneous injections of saline, but the paucity of subcutaneous tissue makes this quite impracticable.

Rectal injections provide the only means whereby a sufficient quantity of fluid may be absorbed. To make this means effective it is necessary to hold the child up by the heels. In this way, and only in this way, can an appreciable amount of fluid be retained. These injections should be very slowly administered, three or four times a day. Success depends on whether the fluid is retained in sufficient amount, and it should not be less than half an ounce.

Of course I do not under-estimate the importance of heat and of colostrum, the latter of which excites the formation of the anti-bodies that are considered necessary for assimilation. Neither do I forget that such children require warmth and nourishment in its most digestible form, such as small quantities of peptonised whey, until the mother's milk is established.

The morbidity shows a further improvement, 3.85 per cent., as compared with 5.30 per cent. by the B. M. A. standard—*i.e.*, a temperature reaching 100° Fahr. on any two occasions between the second and eighth day, by bi-daily taking, the mouth temperature to be recorded, abortion cases to be excluded, and deaths to be included in the morbidity list, regardless of temperature. By our own estimate it is 2.49 per cent., as compared with 3.64 per cent. last year.

Our method of estimating morbidity is as follows:—A patient is considered morbid if her temperature and pulse, taken twice daily, rise above 99° F. and 90 respectively on three consecutive occasions, counting from the beginning of the second day to the time of her discharge, which in normal cases is noon of the eighth day. Abortion cases are included, but deaths without fever are excluded. If the patient's temperature and pulse are above the lines indicated she is not permitted to leave the hospital until they fall to normal for

three consecutive takings, and should a patient be kept in the hospital after the eighth day, and then show any morbidity, our statistics will show it. Temperatures are always taken in the mouth.

We have to record four deaths, which is the smallest number that has occurred during any year of my mastership. Of these four, two were due to sepsis, one to shock, and one to pulmonary tuberculosis.

Two Cæsarean sections were performed during the year; the first calls for no comment, the other was the first of three hysterotomies that I have performed up to date. So far as I know it was the first performed in Great Britain. The steps of the operation were as follow :—

A transverse incision from one anterior superior spine to the other, passing about one inch above the pubes, was carried down to and through the sheath of the rectus. The sheath was separated upwards. The muscles were divided from each other vertically in the middle line. The transversalis fascia and peritoneum were incised transversely. The bladder was raised, putting the vesico-uterine fold on the stretch. This was opened transversely, a finger beneath the fold guarding the uterus from injury. The upper edge of this fold was separated upwards from the uterus and stitched to the upper edge of the wound in the parietal peritoneum, shutting off the general peritoneal cavity. This union was carried all the way around the peritoneal edges, but it is not necessary to continue it any further than just beyond the angles of the wound. Interrupted sutures were used to prevent the closing in of the orifice of the opening which a continuous suture causes.

The lower uterine segment and cervix were opened longitudinally in the middle line. The head was extracted with the lower blade of the forceps and one hand. After removal of the placenta the uterus did not contract well, and hæmorrhage was free. The uterine cavity was douched with hot saline, and plugged with iodoform gauze, the end of which was pushed into the vagina. The uterine incision was closed with interrupted catgut sutures. The free edge of the peritoneum

above the bladder was united by interrupted catgut sutures to the edge of the peritoneum, from which it was originally severed. The aponeurosis was closed with continuous silk sutures, and the skin with Michel's clamps.

My case of symphysiotomy was performed under most unfavourable circumstances for fœtal distress in a woman with contracted pelvis complicated by a brow presentation. The membranes were ruptured, the liquor amnii drained off, and solid meconium coming away. The os was the size of a shilling. After the patient was anæsthetised considerable delay ensued while the os was being opened with Frommer's dilators. The death of the fœtus, though much to be regretted, was not unexpected. The only interest in respect to the operation was its ease of performance and completeness of subsequent recovery. The woman walked from the seventeenth day, and in this respect showed no difference to convalescence after pubiotomy.

L. J., with a conjugate diameter of $9\frac{3}{5}$ cm., delivered herself naturally of twins. The first, weighing 5 lbs., had a large depressed fracture on the right frontal bone. This was quickly and successfully raised by the plan which I introduced in 1904, and have employed on many similar occasions. This method has proved efficient in raising a depression in a child fifteen months old. That it raises both plates without injuring the brain was demonstrated by *post-mortem* on the baby, which was shown to this Section recently.

The sharp point of one blade of a bullet forceps was bored through the bone at the centre of the fracture. The point must be sharp, else the depression is increased. The shank was then turned at right angles to the bone, and the depressed fracture pulled steadily into position.

The case of C. D. raises the question of correct treatment in pregnancy complicated by myoma. These cases are at all times uncertain in their results, but when, as in this case, the tumours grow very rapidly before the child is viable, the uncertainty is intensified. With rest in bed we were able to allow pregnancy to advance to the thirty-fourth week. Then

delivery became imperative, and labour was induced by Krause's method.

The reasons for selecting induction and not Cæsarean section were as follow:—

The tumours were so situated that they did not obstruct delivery. The patient was a primigravida, and one felt that Cæsarean section would probably end in hysterectomy and deprive her of the possibility of further offspring. This conservatism was not warranted by the results. The fœtus lay transversely, the cord prolapsed, and the child died before extraction. The tumours underwent red degeneration, and the patient ran a high temperature for ten days, causing us much anxiety. Subsequent hysterectomy resulted in complete recovery.

We had two cases of spontaneous delivery with brow presentations. One in a primipara requires no special mention. The second case was a primipara, and the baby weighed $6\frac{3}{4}$ lbs. It is remarkable in that the head was born neither flexed nor extended, with the chin posterior. It was simply driven out as a brow, and caused extensive laceration of the perinæum. Such a mechanism in brow presentation is rare.

Having been urged to employ iodoform gauze wrung out in izal as a uterine drain in cases of fever after child-birth, we gave it a very extended trial. We found that it clogged the gauze, did not drain as well as gauze in salt solution or weak carbolic solution, and that it had no compensatory advantages. We have given it up.

We first started to use scopolamin in maternity work in September, 1908, and have used it freely throughout the year.

We have given it to about 100 cases, all primiparæ. There was no maternal mortality, but two children were born dead. Personally we do not think these deaths had any direct connection with the administration of scopolamin. What we particularly want to call attention to is that scopolamin given by the mouth acts as well as when given by hypodermic injection. Cases in which scopolamin has been administered do not require any greater personal supervision than normal

ases, though this supervision is, of course, most desirable when ollecting statistics. My assistants purpose dealing with this ubject in a special paper.

My assistant, Dr. Freeland, has been anxious to ascertain ractically if any better results can be obtained in labour ases by the addition of shaving to our present methods. To emonstrate this he has selected ruptured perinæums requiring epair, and the application of forceps as eminently suited for he comparison, every other one of these cases being shaved. his investigation promises most interesting results, but until very much larger number of cases is forthcoming it will erve no good purpose to publish them.

Labour was induced eleven times—for a dead child four imes, for albuminuria and œdema once, for hydramnios once, or myoma once, and for *ante-partum* hæmorrhage once. here remain three cases in which labour was induced at ull term because of fœtal death on former occasions from nexplained causes when labour occurred spontaneously. ive children resulted in these cases.

There were thirty-four cases of *post-partum* hæmorrhage luring the year. In five cases the pulse rate remained below 00; in nine it was between 100 and 120; in ten it was between 40 and 160; and in six it was over 160. Twenty-two cases ccurred before the placenta left. In twelve of these the lacenta was expressed directly from the uterus; in the other en, a portion or all of the placenta was adherent, necessitating anual removal—three times, of pieces of placenta left after xpression; seven times, of the whole placenta. Twelve cases ccurred after the expression of the placenta, two and a half ours being the longest interval between the end of labour nd the occurrence of hæmorrhage. Six of the thirty-four ases followed operative delivery, necessitating an anæsthetic. he treatment was as follows:—Intra-uterine douche followed y a gauze plug, eleven cases; intra-uterine douche, eight ases; repair of cervical tears, two cases. The others were hecked by catheterisation, expression of the placenta, massage, anual control, and the administration of ergotin.

For the accurate compilation of these Reports I am indebted to my Assistant Masters, Drs. Solomons and Freeland.

Maternal Mortality.

B. K., aged thirty-eight, 12-para ; admitted Nov. 17 ; delivered Nov. 18; died Dec., 1909. Hydramnios; anencephalus; rupture of membranes; sepsis. Admitted Nov. 17, two weeks beyond full term, no fœtal heart heard, membranes ruptured, and fifteen pints of liquor amnii withdrawn ; five and a half hours later she delivered herself naturally of a 5½ lb. macerated anencephalic fœtus. Temperature remained normal until the seventh evening, from which time it gradually rose until death. She received four intra-uterine douches. The uterus was explored manually, and considerable decidual debris was removed from the placental site. There was a diplococcus in the smear and pure culture. She developed mania a few days before death.

See Pathological Report for *post-mortem* findings.

M. M., aged twenty-four, 1-para ; admitted July 31 ; delivered August 3 ; died August 5, 1909. Pulmonary tuberculosis ; prolapse of cord ; forceps. Admitted in the last stages of phthisis. When she entered the second stage the membranes ruptured, cord prolapsed, and a living child 7 lbs. weight was easily extracted with forceps. She died two and a half days later.

See Pathological Report for *post-mortem* findings.

E. M., aged twenty-one, 2-para ; admitted August 3 ; delivered August 4 ; died August 4, 1909. Miscarriage ; rupture of the uterus ; sepsis. On admission severe abdominal pain ; fœtid discharge ; a history of four months' pregnancy. Abdomen very tender. Fœtus half way through the os. Fœtus removed. In removing placenta, uterus was found ruptured on right side; the uterine cavity and rent were plugged with iodoform gauze. Patient at this time showed signs of general peritonitis, and died within ten hours.

See Pathological Report for *post-mortem* findings.

M. M., aged nineteen, 1-para ; admitted August 20 ; delivered August 20 ; died August 20. Twins ; labour shock. Admitted in normal labour. Membranes artificially ruptured when they

appeared at vulva, and patient immediately collapsed after the discharge of a large amount of liquor amnii. She shortly afterwards delivered herself of twins ; there was very little hæmorrhage, but despite free stimulation the patient never rallied, and died three hours after delivery.

See Pathological Report for *post-mortem* findings.

Cases Mentioned Above.

A. W., aged twenty-five, 5-para ; admitted April 6 ; delivered April 8 ; discharged April 28, 1909. Classical Cæsarean section. Operation performed before the onset of labour.. No difficulty. Uninterrupted recovery. Child weighed $7\frac{1}{4}$ lbs. Left the hospital alive. All previous children dead born with fractured skulls.

See table of Contracted Pelves.

M. G., aged thirty-five, 1-para ; admitted October 22 ; delivered October 25 ; discharged Nov. 14. Extra-peritoneal Cæsarean section. In labour fourteen and a half hours, with membranes ruptured when operation performed. Child alive ; $5\frac{3}{4}$ lbs. ; about four weeks premature. Uninterrupted recovery.

See table of Contracted Pelves.

M.D ., aged twenty-three, 1-para ; admitted July 9 ; delivered July 11 ; discharged August 7. Symphysiotomy ; brow presentation ; internal version. In labour ten hours when operation performed.

For details of case, see body of Report.

M. C., aged twenty, 1-para ; admitted August 24 ; delivered August 25 ; discharged Sept. 2, 1909. Twins ; fractured skull. Normal delivery. Children weighed 5 and $4\frac{3}{4}$ lbs. respectively. True conjugate $9\frac{3}{5}$ cms.

For details, see body of the Report.

C. D., aged thirty-seven, 1-para, admitted Jan. 16 ; delivered Feb. 13 ; discharged March 31. Myoma ; induction of premature labour ; transverse presentation ; prolapse of cord ; version ; subsequent hysterectomy. Thirty-two weeks pregnant, multiple

myomata, growing rapidly. Labour induced by Krause's method. Child dead. Transferred to Gynæcological Ward on the twenty-second day. Hysterectomy performed on the twenty-fourth day. Uninterrupted recovery.

S. E., aged twenty-nine, 1-para ; admitted Sept. 14 ; delivered Sept. 14 ; discharged Sept. 22, 1909. Brow presentation ; spontaneous delivery ; chin posterior. Nine hours in labour. No difficulty. Child alive ; $6\frac{3}{4}$ lbs. weight. Circumference of head ; chin to occiput, 14 inches. Extensive laceration of both levators.

This patient was to have returned for internal pelvimetry, but has not done so.

TABLE NO. I.—*Maternity Department.*

—	Nov.	Dec.	Jan	Feb.	Mar.	Apr.	May	June	July	Aug.	Sept.	Oct.	Tot
Total deliveries -	165	152	137	149	178	171	171	169	182	204	166	129	1,97
Total abortions -	4	1	3	5	5	6	5	11	11	8	9	4	7
Not in labour -	26	24	20	27	32	25	32	41	26	32	19	20	32
Total	195	177	160	181	215	202	208	221	219	244	194	153	2,30

TABLE NO. II.—*Nature and Number of Cases Treated in the Extern Maternity.*

Total number of Labours -	2,200	Hæmorrhage—	
Abortions - - -	230	Accidental - -	5
Hydramnios - -	11	Unavoidable - -	6
Myxoma chorii - -	1	*Post-partum* - -	37
		Rupture of uterus -	1
Presentations—		Operations—	
Face to pubes - -	24	Forceps - -	34
Face - -	5	Version - -	18
Brow - -	1	Manual removal -	19
Breech - -	75	Eclampsia - - -	1
Transverse - -	8	Mortality, maternal -	8
		Infantile Conditions—	
Twins - - -	21	Anencephalus -	2
Triplets - - -	1	Hydrocephalus -	3
Prolapse of funis - -	11	Spina bifid - -	1

TABLE No. III.—*Extern Mortality.*

Name	Age and Para	Date of Delivery.	Cause of Death	Time Ill	Remarks
S. U.	40 years, XI.	Nov. 9, 1908	*Post-partum* hæmorrhage	Died almost immediately	Dead when extern assistant arrived
M. N.	— II.	Nov. 27, 1908	Pulmonary embolus	Died 9th day of puerperium; pleurisy for 5 days before death	Got out of bed against orders; collapsed and died
A. M.	44 years, XXV.	Dec. 10, 1908	Acute dilatation of the heart	Died suddenly, 2nd day of puerperium	Patient had chronic endocarditis. Placenta manually removed
M. L.	24 years, I.	Jan. 10, 1909	Septicæmia	41 days	Sapræmia first; membranes and placenta complete
A. T.	— IX.	Feb. 7, 1909	*Ante-partum* hæmorrhage	—	Moribund when friends sent into hospital; dead when extern assistant arrived
E. C.	24 years, II.	May 4, 1909	Shock; *post-partum* hæmorrhage	3 hours	Adherent placenta; manual removal
C. M.	32 years, V.	June 6, 1909	Pernicious anæmia	Died 5th day of puerperium	Was in labour when seen first; hæmoglobin, 10 per cent.; R.B.C., 350,000
L. M.	31 years, II.	Aug. 8, 1909	Septicæmia	14 days	Lacerated perineum; membranes and placenta complete

TABLE No. IV.—*Showing Nature and Number of Cases Treated in Intern Maternity.*

Total admissions	2,369	Mortality—	
,, deliveries	2,045	Maternal	4
Primiparæ	723	Infantile, died in hospital	36
Abortions and miscarriages	72		
Hyperemesis	1		
Hydramnios	5		
Myxoma chorii	2	Stillborn—	
		Recent	29
Presentations—		Macerated	43
Face to pubes	20	Putrid	4
Face	6		
Brow	4		
Breech	46		
Transverse	4	Infantile conditions—	
Twins	29	Anencephalus	1
Impacted shoulders	1	Anencephalus and spina bifida	1
Prolapse of funis	16	Hydrocephalus	4
		Hydrocephalus and spina bifida	1
Hæmorrhage—		Spina bifida	1
Accidental	6	Encephalocele	1
Unavoidable	2	Encephalocele, talipes, supernumerary fingers and toes	1
Post-partum	34	Cephalhæmatoma	7
Laceration of cervix (operative)	7	Fractured humerus	1
		Fractured clavicle	3
Lacerated perineum—		Ophthalmia	4
Primiparæ	345	Talipes	5
Multiparæ	137	Imperforate anus	2
Failure of primary union	18	Umbilical hernia (large)	1
Atresia of vagina	1	Hare-lip	2
Rupture of uterus	1	Cleft palate	1
Hæmatoma of vulva	2	Absence of hand	1
Adherent placenta	15	Depressed fracture of skull	1
Contracted pelvis	14	Dislocation of clavicle	1
		Fibroma of finger	1
Operations—		Genu varum	1
Induction of labour	11	Hæmophilia	1
Episiotomy for central tear	3	Hæmatoma of cheek	1
Cæsarean section	1	Icterus neonatorum	2
Hysterotomy	1	Melæna	3
Symphysiotomy		Nævus	2
Perforation after coming head	1	Rupture of funis	1
Forceps	81	Imperforate anus, absence of ext. genitals	1
Version	17	Monster with teeth, undescended testicles, five fingers and toes, hygroma, &c.	1
Eclampsia	7		
Mania	4		
Phlegmasia	1		
Puerperal ulcer	5		
Morbidity—			
B. M. A. estimation	3.75		
Rotunda estimation	2.49		

Table No. V.—*Accidental Hæmorrhage.*

Age and Para	Variety	Period	Result to Mother	Result to Child	Presentation	Treatment and Remarks
48, XIV.	External	Term	Recovery	Dead	Vertex	Plugged and tight binder; removed in 5 hours; good pains; delivered 1½ hours later
24, I.	,,	8 months	,,	Alive	,,	Forceps when os fully open.
26, I.	,,	Term	,,	,,	,,	Membranes ruptured.
28, V.	,,	,,	,,	,,	,,	Plugs and tight binder; removed 6 hours; good pains; delivered 5 hours later.
31, XII.	,,	,,	,,	Macerated	,,	Plugged and tight binder; removed in 6 hours; almost unaided delivery.
33, VIII.	,,	,,	,,	Alive	,,	Forceps when os fully open.

Table No. VI.—*Placenta Prævia.*

(Includes Extern Cases).

Age and Para	Variety	Period	Result to Mother	Result to Child	Presentation	Treatment and Remarks
42, VIII.	Lateral	7 months	Recovered	A.	Vertex	Spontaneous delivery.
23, III.	Central	,,	,,	D.	,,	Bi-polar version.
25, V.	Marginal	Term	,,	,,	Transverse	Bi-polar version.
30, IX.	Lateral	8 months	,,	A.	Vertex	Bi-polar version.
36, X.	Central	7 months	,,	D.	,,	Bi-polar version.
33, VII.	Marginal	Term	,,	A.	Breech	Foot brought down.
38, VIII.	Central	,,	,,	,,	Transverse	Bi-polar version.
24, II.	Marginal	,,	,,	,,	Vertex	Internal version.

Table No. VII.—*Contracted Pelves.*

All internal measurements made with Skutsch Pelvimeter.

Name	Age	Para	Pelvic Measurements					Mode of Delivery	Result to Child	Weight of Child	Result to Mother	Remarks
			C. V.	Trans.	Ext. C.	I.-C.	I.-S					
J. N.	30	I.	8	10	17	26	25½	Breech normal	Alive	3⅓	Alive	Premature, 7 months. Baby died in 2½ hours.
B. G.	19	I.	8	11	17	25	23½	Vertex normal	Alive	7	Alive	—
E. D.	22	I.	8	10	18½	26	22	Vertex normal	Alive	5¼	Alive	Head fixed late in labour. Walcher's position. Scopolamin and morphin.
A. N.	25	V.	8	11	18½	27	25	Cæsarean Section	Alive	7¼	Alive	Previous children dead born. One prolapse cord and fractured skull. Others forceps and fractured skulls.
M.A.K.	22	I.	7·5	—	17½	26½	25	Brow, bipolar version, perforation	Dead	9	Alive	Prolapse of cord. Aftercoming head stuck. Perforated.
E. G.	26	II.	8⅗	10⅕	18	26½	24	Internal version	Dead	5½	Alive	Prolapsed cord, stopped pulsating when version started. Membranes ruptured some hours and liquor amnii drained away
J. M.	19	I.	8	12	16	28	26	Breech extraction	Alive	7¾	Alive	Prolapse of cord. Breech unfixed. Great difficulty with aftercoming head.
L. B.	35	I.	6	10¾	17½	27½	25¼	Version	Dead	5¾	Alive	Head presenting, cord prolapsed.
M. D.	23	I.	8	12⅘	19	28	26	Symphysiotomy. Version	Dead	6½	Alive	Membranes ruptured with onset of labour.
C. K.	23	I.	8	11½	20	26	25	Normal	Alive	8	Alive	—
C. S.	33	II.	9	12¼	20½	28	25½	Induction of labour, forceps	Alive	9	Alive	First child normally delivered dead.
A. T.	25	I.	7	11½	16	27	26	Breech	Dead	5½	Alive	Macerated.
M. S.	24	III.	8½									

Table No. VIII.—*Forceps.*

Indication	No.	Dead Children	Remarks
Delayed second stage -	63	6	3 macerated ; one twins ; forceps on both.
Delayed second stage. Persistent occipito-posterior.	4	0	
Accidental hæmorrhage	2	0	
Eclampsia - -	1	0	
Mania - - -	1	1	Macerated.
Œdema and albuminuria	1	0	
Pendulous abdomen -	1	0	
Phthisis - -	1	0	
Primary inertia. Fœtal indications.	2	0	
Prolapse of cord -	2	0	
Sepsis - - -	2	2	1 putrid.
Threatened rupture of uterus.	1	1	
Total - -	81	10	5 macerated.

Sub-table A.—*Showing No. of Pregnancy.*

I.-para - - - -	65
II.-para - - - -	12
III.-para - - - -	0
IV.-para - - - -	1
V.-para and over - - -	3
Total -	81

Sub-table B.—*Showing Ages of Patients.*

17-25 - -	27
26-30 - -	27
31-35 - -	18
36 and over -	9
Total -	81

TABLE No. IX.—*Eclampsia.*

Name	Age and Para	Condition on Admission	Urine	No. of Fits			Treatment	Result to Mother	Result to Child	Remarks
				Before Labour	During Labour	After Labour				
C. D., admitted Feb. 2, '09	20 years I.	Conscious	Albumen abundant	–	–	6	Morphin; bowel washed out (2); purgatives; infusion under breasts; poultices to loins	Rec.	A D	Delivered Feb. 2. First fit 9 hours later
C. M., admitted Feb. 18, '09	23 years II.	Conscious	Albumen abundant	–	–	1	Morphin; stomach washed out; purgatives	Rec.	A	Delivered Feb. 18. Fit 1¼ hours later
T. K., admitted June 10, '09	46 years I.	Unconscious	Albumen abundant	8	1	–	Morphin; stomach washed out; bowel washed out; poultices to loins; purgatives	Rec.	D	Delivered June 11
B. I., admitted June 14, '09	34 years I.	Conscious	Albumen abundant	–	–	5	Morphin; bowel washed out (2); purgation	Rec.	A	Delivered June 14. First fit 2 hours after delivery
E. H., admitted June 15, '09	29 years I.	Unconscious	Albumen abundant	8	1	1	Morphin; stomach washed out; bowel washed out; purgatives; poultices to loins; stimulation; oxygen; artificial respiration (3 times)	Rec.	D	Chloral gr. xx. before admission; pulse very poor; cyanosis on admission; three times during day respiration ceased altogether. Delivered June 15
M. N., admitted July 29, '09	40 years II. 1st preg. abortion	Œdema of legs; conscious	Albumen abundant	–	1	–	Morphin; bowel washed out; forceps	Rec.	A	Fit during second stage. Delivered June 29
M. McK., admitted Sept. 30, '09	29 years I.	Unconscious	Albumen abundant	4	–	–	Morphin; bowel washed out; poultices to loins; purgatives	Rec.	D	Child macerated. Delivered Oct. 5

Name	Age and Para	Size of Pelvis.	Result to Child	Weight	Result to Mother	Presentation	Treatment	Remarks
K. M.	35 years—X.	Not measured*	A.	$10\frac{1}{4}$	Rec.	Vertex	Replaced ; forceps -	Delivered 2 hrs. after replacement
C. D.	37 years—I.	Not measured	D.	$6\frac{1}{4}$	Rec.	Transverse	Bi-polar version -	Afterwards had hysterectomy for myoma
M. K.	30 years—V.	Not measured*	A.	8	Rec.	Breech	Traction - -	
B. D.	19 years—I.	Not measured	A.	$6\frac{1}{2}$	Rec.	Breech	Traction - -	First of twins
K. T.	30 years—VI.	Not measured*	A.	$7\frac{1}{8}$	Rec.	Vertex	None - -	
M. K.	22 years—I.	7.5 cm. C. V.	D.	9	Rec.	Brow	Bi-polar version ; perforation.	Cord prolapsed with os size of a shilling
E. G.	26 years—II.	$8\frac{3}{5}$ cm. C. V. $10\frac{1}{5}$ cm. trans.	D.	$5\frac{1}{2}$	Rec.	Vertex	Int. version - -	Membranes ruptured some hours on admission
J. M.	19 years—I.	8 cm. C. V. 12 cm. trans.	A.	$7\frac{3}{4}$	Rec.	Breech	Traction - -	Dislocation of clavicle
L. B.	35 years—I.	6 cm. C. V. $10\frac{3}{4}$ trans.	D.	$5\frac{3}{4}$	Rec.	Vertex	Int. version - -	Membranes ruptured some hours on admission
C. B.	30 years--VII.	Not measured*	A.	$5\frac{3}{4}$	Rec.	Breech	Traction - -	
M. D.	22 years—I.	Not measured	A.	$6\frac{1}{2}$	Rec.	Breech	Traction -	
E. K.	27 years I.	Not measured	D.	9	Rec.	Vertex	None - -	Pulseless ; cord prolapsed when membranes ruptured
A. M.	30 years—VI.	Not measured*	A.	$8\frac{3}{4}$	Rec.	Vertex	Int. version - -	Fractured clavicle
M. M.	34 years—I.	Not measured	A.	7	Died	Vertex	Forceps - -	Phthisis. See Mortality Table
M. P.	28 years—I.	Not measured	A.	7	Rec.	Vertex	Forceps - -	
K. R.	23 years—I.	Not measured	A.	8	Rec.	Breech	Traction - -	

* Five of the cases in primiparæ were not measured because of laceration of the perineum.

TABLE No. XI.—*Breach Presentations.*

Para	Total	Dead Children		Remarks
Primiparæ	16	Fresh	1	Four associated with prolapse of the cord. Two premature, one of which was one of twins. Death during labour was due to primary inertia. Six were one of twins.
		Macerated	2	
		Total	3	
Multiparæ	30	Fresh	0	
		Macerated	2	
		Total	2	
Totals	46		5	4 macerated.

TABLE No. XII.—*Twins.*

Both males - - -	7
Both females - - -	12
One male and one female - -	10
Total - - -	29

TABLE No. XIII. A.—*Morbidity.*

British Medical Association Estimation	Nov.	Dec.	Jan.	Feb.	March	April	May	June	July	Aug.	Sept.	Oct.	
Total Cases -	165	152	137	149	178	171	171	169	182	204	166	129	19
Cases Morbid -	5	3	4	4	9	6	5	7	7	11	8	7	
Percentage -	3.03	1.98	2.92	2.68	5.06	3.51	2.92	4.14	3.84	4.41	4.82	5.43	3

Total number - 76
Total percentage - 3.85

Table No. XIII. B.

Rotunda Estimation	Nov.	Dec.	Jan.	Feb.	March	April	May	June	July	Aug.	Sept.	Oct.	Total
Total Cases -	169	153	140	154	183	177	176	180	193	212	175	133	2045
Cases Morbid -	5	2	3	3	9	4	2	4	4	8	5	2	51
Percentage -	2.96	1.31	2.14	1.95	4.92	2.26	1.14	2.22	2.07	3.77	2.86	1.50	2.49

Total number - 51
Total percentage - 2.49

Table No. XIII. C.—*Comparison of Morbidities.*

PRIMIPARÆ.

British Medical Association Estimation	Nov.	Dec.	Jan.	Feb.	March	April	May	June	July	Aug.	Sept.	Oct.	Total
Total Cases -	59	49	50	46	65	62	69	73	72	80	54	44	723
Cases Morbid	2	3	2	1	6	3	3	4	2	4	3	3	36
Percentage -	3.39	6.12	4.00	2.17	9.23	4.84	4.35	5.48	2.78	5.00	5.56	6.82	4.98

Total cases - 36
Percentage - 4.98

MULTIPARÆ.

Total Cases -	106	103	87	103	113	109	102	96	110	124	112	85	1250
Cases Morbid -	3	0	2	3	3	3	2	3	5	7	5	4	40
Percentage -	2.83	0	2.30	2.91	2.65	2.75	1.96	3.13	4.55	5.65	4.46	4.71	3.20

Total cases morbid - 40
Total percentage - 3.20

TABLE No. XIV.—*Causes of Morbidity other than Uterine.*

Alveolar abscess	1
Bronchitis	1
Constipation	4
Eclampsia	1
Influenza	3
Mastitis	2
Puerperal ulcer	5
Pulmonary tuberculosis	1
Septic thumb	1
Superficial symphysiotomy	1
Suppuration of wound	1
Syphilis	1
White leg	1
Total ...	23

TABLE No. XV.—*Operative Cases, showing Morbidity.*

Forceps (both, 4; B.M.A., 4)	8
Induction of Labour (B.M.A., 2)	2
Manual removal of placenta (both, 2; B.M.A., 3) . . .	5
Rupture of membranes for hydramnios (both, 1) . . .	1
Post-partum hæmorrhage (both, 1)	1
Symphysiotomy (B.M.A., 1)	1
Version (B.M.A., 2)	2
	20

TABLE No. XV. A.—*Percentage Operative Morbidity, B. M. A. Estimation.*

Forceps	9.88 per cent.
Manual Removals	33.33 per cent.

TABLE No. XV. B.—*Rotunda Estimation.*

Forceps	4.94 per cent
Manual Removals	13.33 per cent.

TABLE No. XVI.—*Lacerated Perineums showing Morbidity.*

B.M.A., estimation alone . . .	4
Rotunda „ „	0
Both estimations	8
Total	12

5 of these associated with forceps.

The following Tables are based on those cases morbid by the B. M. A. Estimation :—

TABLE No. XVII.—*Duration of stay in House of Morbid Cases.*

Under 10 days .	33, including 3 deaths.
10–19 days .	34
20–29 days .	9, including 1 death.
Total	76

Table No. XVIII.—*Duration of Temperature.*

Under 5 days	.	57, including 3 deaths.
5–9 days	.	12
10–19 days	.	7, including 1 death.
	Total,	76

Table No. XIX.—*Highest Temperature Recorded.*

100°–100.9°	28, including 3 deaths.
101°–101.9°	25
102°–102.9°	11
103°–103.9°	11
104°–104.9°	1, including 1 death.
Total,	76

Table No. XX.—*Treatment of Morbid Cases.*

No treatment . . .	21
Vaginal douche only . .	3
Uterine douche . . .	39
Quinine only . . .	7
Local treatment (suppurations)	6
Total,	76

Table No. XX. A.—*Uterine Douches.*

20 patients had 1 douche.
9 patients had 2 douches.
7 patients had 3 douches.
2 patients had 4 douches.
1 patient had 6 douches.

Total, 39

Izal was used to soak gauze drains before insertion instead of 2 per cent. carbolic for from 4–6 months, and then given up. Of these 76 morbid cases 23 had incomplete membranes.

For report of uterine cultures see Pathological Report.

Table No. XXI.—*Maternal Mortality.*

Name and Age	Admitted	Delivered	Died	Cause of Death	Remarks
	1908	1908	1908		
B. K., 38 yrs.	Nov. 17	Nov. 18	Dec. 8	Sepsis	Hydramnios; anencephalus. See P. M. report
	1909	1909	1909		
M. M., 24 yrs.	July 31	Aug. 3	Aug. 5	Pulmonary tuberculosis	——
E. M., 21 yrs.	Aug. 3	Aug. 4	Aug. 4	Miscarriage; rupture of uterus; sepsis	Delivered just after midnight; died before midday
M. M., 19 yrs.	Aug. 20	Aug. 20	Aug. 20	Labour shock	——

STATISTICAL RETURNS OF THE GYNÆCOLOGICAL DEPARTMENT.

TABLE I.—*Showing nature and number of Operations.**

VULVA AND PERINEUM—	
Carcinoma of vulva (2)—	
Adenocarcinoma	1
Epithelioma	1
Hymenectomy	2
Lacerations of Perineum (86)—	
Complete	6
Incomplete	80
URETHRA AND BLADDER—	
Prolapse of urethral mucous membrane	1
Urethral caruncle	3
VAGINA—	
Cicatrix	1
Cyst	2
Fistula, vesico-vaginal	1
Anterior colporrhaphy	17
Anterior colpotomy	8
Posterior colpotomy	5
RECTUM—	
Fissure of rectum cured	1
Hæmorrhoids	5
Prolapse of rectum	1
CERVIX—	
Trachelorrhaphies and amputations (75)—	
Schroeder	57
Sanger	14
Emmet	3
Circular amputation	1
Posterior division	27
Pozzi's operation	2
Carcinoma	6
UTERUS—	
Curettage	254
Polypus (fibroid)	13
Ventro-suspension	82†
Vaginal suspension	8
Ventro-fixation	4
Alexander Adams	4
Myomectomy (12)—	
Abdominal	11
Vaginal	1
Hysterectomy (31)—	
Supra-vaginal	19
Wertheim	7
Complete	5
Cæsarean section	1
TUBES AND OVARIES—	
Ovariotomy	14
Resection of ovary with other operations	19
Resection of ovary	2
Resection of tube with other operations	25
Salpingo oöphorectomy with other operations	13
Salpingectomy	2
Salpingo-oöphorectomy	12
Tubal pregnancy	7
Parovarian cyst	4
MISCELLANEOUS—	
Herniotomy (4)—	
Inguinal	2
Umbilical	2
Appendicectomy	1
Appendix abscess	1
Nephrectomy	1
Pylorectomy	1
Gastroenterostomy	1
Laparotomy	6
Parametritis	1
Talma-Morison	1

* 168 Abdominal Sections were performed.

† Of the Ventro-suspensions 35 were unaccompanied by any other Abdominal Operation.

TABLE No. II. A.—*Abdominal Hysterectomies.*

No.	Name	Date	Age	Disease	Operation	Result	Remarks
1	E. D.	7/1/'09	39	Degenerate fibroid, pyosalpinx	Supravaginal hysterectomy	Recovered	Married, multipara; plugged
2	M. W.	5/1/'09	38	Fibroid uterus	,,	,,	Married, nullipara; both ovaries left
3	C. D.	16/2/'09	37	,,	,,	,,	Married, nullipara; left tube and ovary removed
4	M. O.	16/2/'09	37	General disease of uterus and adnexa	,,	,,	Both tubes and ovaries removed; married, 2-para
5	M. A.	19/2/'09	23	Pyosalpinx -	Complete hysterectomy	,,	Married, nullipara; both tubes and ovaries removed; plugged
6	C. D.	26/2/'09	28	,,	,,	,,	Married, nullipara
7	J. W.	5/3/'09	48	Fibroid uterus -	Supravaginal hysterectomy	,,	Married, 4-para; both tubes and ovaries left
8	M. S.	1/6/'09	34	,,	,,	,,	Unmarried
9	M. M.	6/4/'09	40	,,	,,	,,	Both tubes and ovaries removed
10	A. B.	4/6/'09	43	Intraligamentary myoma	,,	,,	Unmarried; tubes and ovaries left
11	L. B.	12/6/'09	44	Fibroid uterus -	,,	,,	Married, multipara; tubes and ovaries removed
12	M. G.	22/6/'09	43	,,	,,	,,	Married, nullipara; tubes and ovaries left
13	K. M.	15/6/'09	49	Ovarian cyst malignant; secondary deposits in uterus and ovariotomy	Complete hysterectomy	,,	Married, nullipara; other ovary removed for cyst 1½ years previously

TABLE No. II. A.—*continued.*

No.	Name	Date	Age	Disease	Operation	Result	REMARKS
14	C. C.	25/6/'09	42	Fibroid uterus -	Supravaginal hysterectomy	Recovered	Unmarried; one ovary removed
15	C. M.	18/6/'09	44	,,	,,	,,	Unmarried; both tubes and ovaries left
16	B. H.	19/7/'09	44	,,	,,	,,	Married, nullipara; both tubes and ovaries left; adhesions
17	J. R.	22/7/'09	50	Fibroid uterus	,,	,,	Unmarried; both tubes and ovaries removed
18	A. M.	23/7/'09	45	,,	,,	,,	Married, nullipara; tubes and ovaries left
19	M. B.	—	36	Retro-peritoneal tumour	Complete hysterectomy	,,	Unmarried; operation to reach tumour
20	J. C.	10/9/'09	42	Fibroid uterus -	Supravaginal hysterectomy	,,	Married, multipara; double salpingo-oöphorectomy
21	D. B.	25/9/'09	38	Bilateral pus tubes -	Supravaginal hysterectomy; double salpingo-oöphorectomy	,,	Very ill, with symptoms of peritonitis for 48 hours after operation; plugged
22	E. G.	21/9/'09	40	Fibroid uterus -	,,	,,	Unmarried
23	G. N.	2/10/'09	40	,,	,,	,,	Two children last pregnancy; abortion 8 years ago
24	M. F.	15/10/'09	36	Double pyosalpinx -	Panhysterectomy -	,,	Last pregnancy 5 years ago; plugged

TABLE NO. II. B.—*Abdominal Hysterectomies*—MALIGNANT DISEASE.

No.	Name	Date	Age	Disease	Operation	Result	Remarks
1	J. D.	12/2/'09	60	Adeno-carcinoma of body	Wertheim's	Recovered	Menopause 9 years ago; 4-para, last 27 years ago
2	M. R.	27/4/'09	45	Epithelioma of cervix	,,	,,	6-para, last 6 years ago; recurrence in 5 months
3	M. R.	25/5/'09	49	,,	,,	,,	4-para, last 5 years ago
4	L. R.	28/5/'09	60	,,	,,	,,	1-para, 31 years ago
5	M. R.	8/6/'09	38	,,	,,	Died 11/7/'09. *See* Table VI., No. 6	Nullipara, 15 years married
6	B. O'C.	13/7/'09	44	,,	,,	Died in 3 hours; shock	6-para, last 5 years ago
7	K. W.	13/10/'09	26	,,	,,	Recovered	6-para, last one year ago

TABLE NO. III.—*Myomectomies (Abdominal).*

No.	Name	Age	Date	Disease	Result	REMARKS
1	B. M.	35	13/11/'08	Multiple myomata (8)	Died 18/11/'09	Operated in 12 hours for secondary hæmorrhage; died 5 days later; symptoms of pulmonary embolus; *post mortem*, see Pathological Report. One abortion
2	M. O'S.	20	20/11/'08	Myomata, $2\frac{1}{2} \times 3\frac{1}{2}$ in. on anterior wall	Recovered	Nullipara; 11 years married; ventro-suspension followed myomectomy
3	M. N.	40	11/12/'08	Pedunculated myoma, 12×10 in., on right side	,,	4-para; last 20 months ago
4	T. B.	32	29/12/'08	One large submucous and three small subserous myomata	,,	Uterus split in two and then sewn up again; nullipara; married 6 months
5	M. H.	40	29/1/'09	Myoma, 2×1 in., on fundus	,,	Unmarried; ventrosuspension and amputation of cervix also performed
6	C. B.	38	5/2/'09	Myoma, 3×3 in., on anterior wall	,,	One abortion 3 years ago, followed by ventro-suspension
7	M. M.	34	12/3/'09	3 myomata; subserous right cornu 5 in. in diam., another $\frac{1}{2}$ in. diam., left cornu; third interstitial anterior wall, 1 in. diam.	,,	Unmarried; followed by ventro-suspension.
8	B. K.	36	16/4/'09	One large and 4 small myomata	,,	1-para, 10 months ago
9	C. H.	36	20/4/'09	One myoma, 8×6 in.; 6 myomata, ranging from $\frac{1}{2}$ to 2 in. in diam	,,	Married one year; nullipara
10	M. H.	29	26/7/'09	One submucous myoma, 5×4 in.	,,	Unmarried; uterus split in two; repaired; then suspended
11	K. H.	35	29/10/'09	One subserous myoma,	,,	Unmarried; followed by ventro-suspension

TABLE No. IV.—*Ovariotomies and Salpingectomies (Abdominal).*

No.	Name	Age	Date	Disease	Operation	Result	Remarks
1	L. M.	27	27/11/'08	Tubal pregnancy -	Left salpingo-oóphrectomy	Recovered	1-para
2	J. B.	26	4/12/'08	Salpingo-oöphoritis -	Right oöphorectomy; partial double salpingectomy	,,	3-para; plug for hæmorrhage
3	M. B.	22	8/12/'08	,,	Partial right salpingo-oöphorectomy	,,	2-para; ventro-suspension performed
4	M. K.	42	17/12/'08	Double pyo-salpinx -	Right salpingo-oöphorectomy; partial left salpingo-oöphorectomy	,,	12-para; drained
5	C. L.	37	8/1/'09	Right ovarian cyst -	Right ovariotomy -	,,	2-para; patient was 4 months pregnant, and later was delivered of a 7½ lb. child; a year after this died of generalised carcinoma. The cyst was benign
6	M. F.	41	19/1/'09	Double ovarian cyst -	Partial double oöphorectomy	,,	11-para; ventro-suspension performed
7	R. H.	26	9/2/'09	Right ovarian cyst -	Right ovariotomy -	,,	1-para; patient was nearly 3 months pregnant, and aborted 12 days after the operation
8	A. M.	24	5/2/'09	,,	,,	,,	Nullipara; simple cyst removed in its entirety
9	M. B.	30	9/2/'09	Ruptured tubal pregnancy	Double salpingectomy (partial)	,,	6-para; ruptured tubal pregnancy on one side; thickened tube on other
10	A. T.	30	19/2/'09	Right ovarian cyst -	Right ovariotomy -	,,	Nullipara; simple cyst

* *See* Footnote to Table I.

TABLE No. IV.—*continued.**

No.	Name	Age	Date	Disease	Operation	Result	REMARKS
11	B. M. Q.	29	2/4/'09	Ruptured tubal pregnancy	Right salpingectomy -	Recovered	5-para; wound suppurated
12	[illegible]	[illegible]	19/3/'09	Left ovarian cyst -	Left ovariotomy -	,,	3-para; right ovariotomy 3 years previously
13	M. H.	38	26/3/'09	Right ovarian cyst; left parovarian cyst	Right ovariotomy; removal of left parovarian cyst	,,	Nullipara
14	M. C.	46	23/4/'09	Left pyosalpinx; appendicitis	Left salpingectomy; appendicectomy	,,	5-para; drained
15	M. C.	23	23/4/'09	Tubal pregnancy -	Right salpingectomy -	,,	2-para; right ovary resected
16	B. F.	51	21/5/'09	Fibroma of ovary -	Right oöphrectomy -	,,	Single
17	J. M.	30	16/5/'09	Tubal pregnancy -	Right salpingectomy -	,,	4-para
18	J. M.	22	1/6/'09	Double salpingo-oóphritis	Left salpingo-oöphorectomy; partial right salpingo-oöphorectomy	,,	Single
19	M. H.	40	7/5/'09	Ovarian cyst -	Right ovariotomy -	Died	Papillary cyst undergoing septic change. *See* Mortality Table VI., No. 5
20	M. C.	51	19/6/'09	Salpingo-oöphritis -	Left salpingo-oöphorectomy	,,	10-para
21	L. J.	25	12/6/'09	Tubal pregnancy -	Partial right salpingectomy	,,	1-para; drained
22	L. B.	34	9/7/'09	,,	Left salpingectomy -	,,	9-para; drained

* *See* Footnote to Table I.

TABLE No. IV.—*continued.**

No.	Name	Age	Date	Disease	Operation	Result	Remarks
23	A.M.	33	2/7/'09	Left ovarian cyst -	Left ovariotomy -	Recovered	Single; simple cyst
24	E. C.	52	19/7/'09	,,	,,	Died (from perforated gastric ulcer after uneventful convalescence)	A large tumour, evidently carcinoma of stomach, discovered during operation; patient died 9 days after ovariotomy; *post mortem* showed perforated gastric (carcinomatous) ulcer. *See* Table VI., No. 9.
25	B. O'D.	24	20/8/'09	Right salpingo-oöphoritis	Right salpingo-oöphorectomy	Recovered	1-para
26	J. M.	26	20/8/'09	Right ovarian cyst -	Right ovariotomy -	,,	Single
27	A. D.	22	31/8/'09	Right salpingo-oöphoritis	Right salpingo-oöphorectomy	,,	Single
28	R. M.	28	31/8/'09	Left pyosalpinx -	Left salpingo-oöphorectomy	,,	Married, nullipara; drained
29	A. D.	44	3/9/'09	Cyst adenoma, right side	Right salpingo-oöphorectomy	,,	5-para
30	J. H.	37	28/9/'09	Left ovarian cyst; right salpingitis	Left salpingo-oöphorectomy; right salpingectomy	,,	2-para
31	M. L.	20	2/10/'09	Left ovarian cyst -	Left ovariotomy -	,,	Single; simple cyst

* *See* Footnote to Table I.

TABLE No. V.—*Miscellaneous Operations.**

No.	Name	Date	Age	Disease	Operation	Result	REMARKS
1	M. B.	12/1/'09	32	Inguinal hernia	Herniotomy	Recovered	Alexander Adams operation performed also.
2	E. B.	29/1/'09	43	Gastric carcinoma	Pylorectomy Gastro-enterostomy	Recovered	Cholecystectomy performed 12 months previously.
3	M. L.	4/2/'09	47	Hypernephroma	Nephrectomy	Died (suppression of urine)	*Post mortem* showed other kidney in a state of chronic intestinal nephritis, also secondary nodule in lung.
4	K. M.	10/4/'09	39	Inguinal hernia	Herniotomy	Recovered	
5	M. F.	4/5/'09	33	Umbilical hernia	,,	,,	
6	M. C	23/4/'09	46	Appendicitis	Appendicectomy	,,	Salpingectomy performed also.
7	M. E.	14/5/'09	30	Tubercular peritonitis	Laparotomy	Relieved	Abdomen studded with tubercle.
8	M. M'L	14/5/'09	27	Cirrhosis of liver, &c.	Talma-Morison	,,	
9	J. R.	16/7/'09	48	Pelvic carcinoma	Laparotomy	Died	*See* Mortality Table VI., No. 8.
10	L. W.	14/9/'09	46	Umbilical hernia	Herniotomy	Recovered	

* This only includes important major miscellaneous operations.

TABLE No. VI.—*Mortality.*

No.	Name	Age	Date of Operation	Died	Cause of Death	Remarks
1	B. M.	35	13/11/'08	18/11/'08	Pulmonary embolism -	*See* Table III., No. 1.
2	M. L.	47	4/2/'09	5/2/'09	Hypernephroma - -	*See* Table V., No. 2.
3	M. K.	36	—	10/3/'09	General purulent peritonitis, empyema right side	Came to the Hospital moribund, and was admitted.
4	C. B.	47	22/3/'09	23/3/'09	Purulent peritonitis -	Patient was very septic on admission. A submucous myoma was removed after splitting the posterior lip, but patient died on the following day. *Post mortem* showed abdomen full of pus ; abscess in right broad ligament.
5	M. H.	40	7/5/'09	8/5/'09	Acute purulent peritonitis -	*See* Table IV., No. 19.
6	M. R.	38	8/6/'09	11/7/'09	Sepsis - -	A very advanced case of carcinoma. *See* Table IIB., No. 5.
7	B. O'C.	44	13/7/'09	13/7/'09	Shock - -	*See* Table IIB., No. 6. *Post mortem* showed no evident carcinoma anywhere. Patient died shortly after operation was completed.
8	M. O'R	48	16/7/'09	23/7/'09	Purulent peritonitis -	Pelvis found completely involved in carcinoma ; abscess cavities broken into at operation. *See* Table V., No. 9.
9	E. C.	52	19/7/'09	28/7/'09	Perforated gastric ulcer -	Ovariotomy performed nine days previously when carcinoma in stomach was discovered, but operation was deferred. Patient, however, suddenly died. *See* Table IV., No. 24.

ABSTRACTS.

SECTION OF OBSTETRICS.

Friday, November 5, 1909.

The President, Dr. Jellett, in the Chair.

Exhibits.

Multiple Myoma.

Dr. E. Hastings Tweedy exhibited a specimen of multiple myoma of the uterus. In such cases one would, if possible, perform a myomectomy. But in the case before them it would have been very difficult, as the uterus was absolutely riddled with tumours. It appeared as a smooth myomatous uterus, but on splitting it they could hardly see any muscular structure, and each of the two or three hundred myomas was absolutely distinct, some degenerating, some not. The patient had a temperature, and macroscopic specimens had the appearance of malignancy, but Dr. Rowlette told him he could find no malignant disease.

Wertheim's Operation.

Dr. Tweedy exhibited a specimen of an ordinary Wertheim's operation in a girl about twenty-four years of age. There was very considerable ulcerative cancer spreading into the broad ligament. The operation went well until he began to remove the cervical part of the uterus. The cancer then broke away, and he got the end of the vagina. He was able to shut off the intestines, and the woman did not become infected. What was particularly interesting was that he traced up the ureter, and found that while the cancer spread above and below the ureter, it was not diseased. The ureter was so intimately associated with the cervix that in dealing with it one had to hug the cervix to

avoid the ureter. He pushed down the bladder far into the vagina, and got his finger between the vagina and bladder. He worked the bladder laterally, and was able to dig the ureter completely out of the cancer, and took away the microscopic evidence of the disease. That was three weeks ago. The patient had gone on very well until a week ago, when her temperature ran up, and a fœtid discharge arose. Though she could now hold a certain amount of urine in the bladder, still a quantity passed away, and he had no doubt he was now dealing with a ureteral fistula. In the majority of cases such fistulæ healed spontaneously; but from the accounts which he had read on the subject he found that the ureter practically never became patent again. The urinary discharge would dry up. The ureter cicatrised, and the kidney ceased to secrete. They would get a hydronephrosis only so long as there was some fluid passing through the ureter. If they did not allow a drop to escape through the ureter, the kidney would not secrete urine; and his hope was that the cicatricial matter would absolutely occlude the ureter, and the kidney would become inert.

Sir William Smyly said ureteral fistula was a risk that anyone was liable to who undertook Wertheim's operation. Recently, when assisting in a case where the ureter had been cut accidently and where implantation into the bladder was impossible, he had persuaded the operator to ligature it with silk and fix it in the abdominal wound. In that case it was found necessary afterwards to extirpate the kidney.

The President of the Academy said he had once transplanted the ureter into the bladder, and as far as he knew no leakage occurred, and no obvious kidney tumour developed. He would be interested to see the result in Dr. Tweedy's case, as it was difficult to understand why hydronephrosis should not occur when the ureter was completely blocked whereas it occurred where the obstruction was only partial.

Dr. FitzGibbon recalled a case of Sir Arthur Macan in which the urine continued to be secreted as freely as ever from the kidney belonging to the detached ureter, and showed no sign of cicatrisation of the cut end of the ureter, or blocking off of the kidney.

Dr. Jellett said one could understand that there was a

kind of race between the function of the kidney and the destruction of the substance by back pressure, and he would be inclined to think that hydronephrosis would result before the destruction of the kidney substance.

Dr. Tweedy, in replying, said that he had in his remarks exactly summed up the conclusions of the experiments that he had read, the last being from the Johns Hopkins Hospital within the last six months. The ureter was simply cicatrised over, there was no patency, and the kidney ceased to secrete. Sir William Smyly's case was not comparable with the condition of spontaneous healing. It was almost impossible to get the ureter to unite permanently by tying.

Pyosalpinx.

Dr. Purefoy exhibited a specimen of pyosalpinx, which had reached a very considerable size in a short time, and had arisen under circumstances which made its origin difficult to account for. The patient had been married several years, and was childless. She had lived abroad, and had had malarial fever and other illnesses incident to residence in a tropical climate. She came under his care for a trifling catarrh of the uterus, and became, as he believed, cured. She went to the country, for a few weeks, where she was attacked with what she thought to be malarial fever, with severe abdominal pains and tenderness. On her return to town it was quite easy on palpation to find a tumour taking its origin from some of the pelvic organs. He waited for six weeks before operating. On opening the abdomen a rounded tumour came into view completely concealing the uterus, to which it was extensively adherent. In separating it, to his great distress the sac ruptured, and a quantity of pus escaped. He was, however, happy to add that the patient had an uninterrupted recovery. There was no ground for thinking the condition to be due to any gonorrhœal affection. There was no evidence of tubercular disease, still he took it that it originated in tubercular salpingitis. She had no signs of tuberculosis, but her family history was unfavourable.

Dr. Tweedy said it seemed to him worthless to leave a uterus that could be of no use when the tubes were gone. When a tube burst, and pus flowed into the pelvis, it was largely a matter of

chance if the patient would recover. If the pus was septic, he knew of no way to avoid the giving of sepsis. If there was any doubt as to the nature of the pus, it would, he thought, be safer to cut away the uterus, and the tube at the other side: this would have provided a large hole for free drainage. That hole and the vagina should be packed with a large quantity of iodoform gauze, and the gauze spread out right over the pelvis whither the pus had flown, and to which the intestines would come the moment the patient was moved out of position.

Sir W. Smyly said he had taken out one tube, and the woman had become pregnant afterwards. He did not think it good practice in every case to remove both tubes and uterus, but where there was soiling of the pelvis and where both tubes had to be removed, he considered it good practice to remove the uterus also.

Dr. Horne said he had recently adopted the practice recommended by Dr. Tweedy in a similar case, the patient did very well up to the fifth day, when she complained of pain in the region of the heart, and rapidly sank in a few hours; he wished to ask Dr. Purefoy what treatment he adopted when the pus escaped into the abdominal cavity?

Dr. Jellett thought it difficult to say positively that the case was tuberculous, and he suggested that it originated as one of extra-uterine pregnancy, and that a hæmatoma formed in it and suppurated. He was afraid he was in a minority in thinking it good practice to leave the uterus even if both tubes were removed. If any portion of the ovaries was left behind, menstruation would continue, which was an important matter from the point of view of the mental comfort of the patient.

Dr. Purefoy, in reply, said it was possible to lift the tumour out of the abdomen to some extent. He had packed the surrounding area with sponges, and the patient escaped contamination from the fact that the adjacent structures had been protected, and the point of exit was outside the incision, and somewhat out of the scene of operations. His recollection was that there was no palpable disease of the other tube, and the case was so serious that he was glad to avoid any unnecessary removal of other parts, and to conclude the operation so far as was compatible with the safety of the patient.

President's Address.

THE PRESIDENT read his Address on "The Teaching of Practical Midwifery in the past and at the present time." See p. 209, *ante*.

Friday, December 3, 1909.

The President, DR. JELLETT, in the Chair.

Exhibits.

Myomatous Uterus.

DR. GIBSON exhibited a specimen of myomatous uterus with necrosis of one large tumour and carcinoma of the endometrium. The patient, from whom the uterus had been removed a few weeks previously, was over sixty years of age, and twelve years past the menopause. She knew that she had the tumour for many years, and had been given to understand that it would never do her any harm after the change of life. She had never been pregnant, and enjoyed good health up to four weeks before he saw her, when she suffered from a severe attack of uterine hæmorrhage. She had not had any discharge from the menopause to that time. She had no pain, and except that she thought she had been getting thinner for the past five or six months, she noticed no change in her general health. Examination showed a large myomatous uterus with the cervix unaltered. There was no hæmorrhage, but when the sound was passed the uterine cavity bled freely. He decided to perform panhysterectomy. When the abdomen was opened, as the uterus felt peculiarly soft it was grasped with a museau-forceps in what seemed to be the most solid part. The forceps, however, tore a hole in the surface of the uterus, and a large quantity of yellowish creamy fluid escaped. Fearing that the uterus would tear, it had been carefully packed off, so that none of this fluid got into the peritoneal sac. It proved, however, not to be pus. The removal of the uterus was easy, but scattered everywhere over the pelvis he found nodules which were deposits of carcinoma. These and the condition of the uterus showed the great extent to which the disease had

extended without any symptoms. The patient did very well. He also exhibited a specimen of carcinoma of the cervix removed by Wertheim's operation. The patient was twenty-five years of age; married four years. Four months before he saw her she had a perfectly normal labour, and suffered from nothing either before or after her babe was born. She had no bleeding until six weeks before he saw her, when she described a creamy discharge with streaks of blood. The disease in the cervix was advanced, and the specimen showed the very large amount of tissue which could be removed with the uterus by this operation. The patient made a good recovery, and was the fourth under the age of thirty for whom he performed Wertheim's operation this year.

Sir William Smyly said it was very difficult to prophesy in myoma. He recalled a case in which the patient had had a uterine tumour for thirty years. Six months after seeing her it was fixed in the pelvis with malignant disease.

Professor Alfred Smith cited a case in which, after some hesitation as to giving advice, he dilated the uterus, and found what he thought to be a tiny ulcer. He scraped away sufficient for a microscopic examination. The pathologist pronounced it to be malignant disease, rapidly growing, in which the prognosis would be very bad. It was, however, a typically ideal case for good results, and he removed the uterus, but within nine months the patient was dead. The case brought home to him the fact that the useful information given by the pathologist could be extended to state whether the tumour was actively growing or not.

The President (Dr. Jellett) said the opinion given twenty years ago as to a tumour not doing harm might have been perfectly correct then, in view of the risk of operation; but now it is possible and right to advise the performance of operation on account of the great improvement in their technique.

Ovarian Pregnancy.

Dr. E. Hastings Tweedy exhibited a specimen of ovarian pregnancy, probably the first exhibited in Ireland. A. H., aged twenty-five; married four years; three children, last five and a half months ago (June, 1909); one abortion. Menstruated September 18, 1909, first and only time since delivery.

November 5th.—Acute pain in right lower abdomen, from which she almost fainted. Examined at 4 p.m. November 5th in extern department. Very tender mass the size of a hen's egg to the right of retroverted uterus. Left appendages normal. Tumour on right could not be definitely localised to ovary or tube.

Diagnosis.—Extra-uterine pregnancy.

November 13, 1909.—In the interval had several attacks of pain. Morning of November 13 pain more severe, and associated with slight uterine hæmorrhage. On admission, tumour increased to twice its former size, otherwise physical examination the same.

Operation.—November 16, 1909.—Ether. Right Oöphorectomy. Both tubes and left ovary microscopically perfectly normal. Blood clot about three inches in diameter completely obscured right ovary. This was clamped and removed, ovarian tissue being cut through in removal.

The case had fulfilled all the conditions laid down for ovarian pregnancy. He thought the specimen was worth public acknowledgment by the Section.

Syphilitic Elephantiasis.

Dr. Tweedy also exhibited a specimen of elephantiasis of the vulva, specific in origin. M. B., aged thirty; married fourteen years; last pregnancy ten years ago; three dead-born children; one child lived two hours. Wassermann's reaction for syphilis positive. (Reported from Sir A. Wright's Laboratory.) Has always had a yellowish purulent vaginal discharge. For ten years has had a tumour of the vulva, which has grown slowly but steadily.

Physical Examination.—Hypertrophy of labia minora, measuring about two and a half by three and a half inches. Considerable œdema. Induration and œdema involved labia majora, and extended down to ischio-rectal fossa on either side.

Operation.—Excision. Interrupted catgut sutures.

Dr. Rowlette said the specimen of ovarian pregnancy showed to the naked eye the *corpus luteum*. A section taken through the *corpus luteum* and the edges of the blood-clot showed a proliferation of the cells of the *corpus luteum* and the mass of blood-

clot in which three or four villi were seen. The other specimen consisted of lax connective tissue covered with a thin skin. The laxity was probably due to an increase of the lymphatic spaces.

Dr. FitzGibbon quoted a case reported in the November number of *The American Journal of Obstetrics.* On the day the woman expected to be delivered she complained of pains. These went off, and twelve days later she went into hospital. A tumour was found strongly resembling an ovarian cyst. On opening into it a fœtus was found. This was delivered and resuscitated. The child was up to term, and free from deformities, and the patient made a complete recovery.

Dr. Jellett said the case was one in which they must suppose a person capable of making a mistake until he could conclusively prove that he had not made one. It was not a question of distrusting the powers of diagnosis of Dr. Tweedy and Dr. Rowlette, but rather a question as to whether the whole profession would accept the power of the Section to express an opinion. He, therefore, urged Dr. Tweedy to refer the specimen to a committee. His own opinion was that it was an ovarian pregnancy.

Dr. Tweedy, in reply, said he was quite agreeable that the diagnosis should be confirmed.

Sir William Smyly proposed that Dr. Tweedy's specimen be sent to the Reference Committee for report.

Professor Alfred Smith seconded, and the motion was passed unanimously. (For report, see meeting of April 1, 1910).

Painless Labour.

Dr. Spencer Sheill read a paper on above subject. See p. 222, *ante.*

Friday, January 14, 1910.

The President, Dr. Jellett, in the Chair.

Exhibits.

Fibroid Myomata.

Professor Alfred Smith exhibited a specimen of fibroid myoma. The patient had come to him in October last greatly dis-

tended, with a note from the doctor stating that he had tapped her a week previously in order to enable her to come to hospital. The patient told him she had been in the hospital fifteen years previously. He looked up the record, and found she had come in with a tumour about the size of a four and a half months' pregnant uterus. She had complained of no symptoms, menstruation was normal, she had no inconvenience whatever, and, according to the teaching then in vogue, he advised her to go home, as there was no reason for her to risk an operation. On the present occasion he did an exploratory operation to ascertain why there was such enormous distention. To his astonishment he found the tumour very little increased in size. It was a fibroid myoma of the entire uterus, and attached to it were great omental adhesions. Cullen, in his book, called it a parasitic fibroid myoma, because it obtained its major blood supply from sources outside the uterus, and stated that in parasitic myoma the omentum became attenuated. In the specimen before them they would observe that the fragment of omentum was big and fat, although the myoma was parasitic. A pathologist had told him that the specimen had undergone sarcomatous degeneration. The chief point of interest was that the case brought home to them the lesson that the present day teaching as to interfering operatively in cases of fibroid myoma were perfectly justified.

Professor Smith also exhibited a specimen of calcified fibro-myoma, or uterine stone. It lay to the right of the uterus, and he thought it was a small ovarian tumour.

Dr. Purefoy said the case illustrated the wise change which had been made in the treatment of fibro-myoma, and gave examples from his own experience in support of the present practice.

Dr. Kidd said it occurred to him that it was quite possible for adhesions to take place between the omentum and the uterus without having to look on the tumour as altogether parasitic.

Dr. Jellett said that calcified myomata of the uterus possessed a certain amount of interest to others than gynæcologists. He recently found a patient with prolapse who had a similar myoma in the middle line behind the symphysis. Dr. Watson, on seeing the case, remarked that the tumour could cause a considerable amount of confusion in the minds of X-rayists, as it might be mistaken for stone in the bladder.

Dr. E. Hastings Tweedy showed :—

1. *A Case of Ovarian Cyst and Pregnancy.*

M. E. N., aged twenty-eight, 2-para, was admitted on December 25, 1909. Previous labour had forceps. She last menstruated on March 17, 1909. On examination there was found a tumour, cystic in consistence, reaching to the ensiform cartilage, overlying a pregnant uterus. An oblique groove, running across the abdomen two and a half inches below the umbilicus, separated the tumour from the uterus. There was a marked fluid thrill over the tumour, the greatest circumference of which was forty-five inches. Tympany was present in the flanks. A fœtal heart was audible. Diagnosis of ovarian cyst and pregnancy was made. Ovariotomy was performed three days later, without complication, when twenty-five pints of clear, almost watery, fluid were evacuated. A full term uterus was evident. Thirty hours later the patient fell into labour, and was delivered by forceps when the second stage had started of a 7¾lb. child.

2. *A Case of Hypophrenic Abscess complicating Pregnancy.*

P. M., aged twenty-five, primipara, was seen in the extern maternity, and being deemed a General Hospital case was advised to go to one, but having tried one of these institutions was there advised to come to the Rotunda. She had been in a general hospital suffering from pneumonia until a week before Christmas. She was in great pain on admission to the Rotunda on January 5, and had been so from shortly after the time of her discharge from the general hospital. There was no history of gastric or renal trouble. On examination it was found that there was a tense resistant mass, dull on percussion, and well defined in its lower margin, which stretched from beneath the left costal arch across the middle line to the right costal arch, occupying the left hypochondrium, the epigastrium, and to a slight extent the right hypochondrium. The uterus was the size of a six months' pregnancy, and there was no communication whatever between the uterus and the growth. Mr. Heuston agreed that the diagnosis was hypophrenic abscess. An exploratory incision was made under local anæsthesia, and a general anæsthetic was given, when it was found necessary to enlarge the incision to below the umbilicus. The peritoneum was thickened and inflamed. A mass, cystic in

appearance, which, when opened, was found to contain, firstly, a watery fluid followed by an immense amount of pus was apparent. The intestines were kept back by sponges, but a good amount of foul-smelling, purulent fluid escaped. The sac of the abscess was carefully irrigated with saline, and then the peritoneal cavity. A Keith's tube and CHI_3 gauze were placed in the abscess sac and CHI_3 gauze in front of the stomach. The sac seemed to have no connection whatever with any viscus. The patient suffered from shock after the operation, and needed a good amount of stimulation. Irrigation, with continuous saline, was commenced soon after the operation was finished, a pint being given for the first hour, half a pint for the next three hours. It returned after this, so was discontinued. Antistreptococcic serum 10 cc.s were given. Twenty-eight hours later patient started in labour, and was delivered by forceps of a six months' fœtus three hours afterwards. Forceps were applied when the os was fully dilated, owing to excessive straining. The placenta followed in due course. The convalescence so far has been very stormy. The Keith's tube was removed on the day following the operation, and was replaced by a small Kocher's tube; this was changed daily until the fourth day. The CHI_3 gauze was removed with difficulty on the third day. A great quantity of discharge came away on the dressings. Great difficulty was experienced in getting the bowels moved and in finding food which would not make her vomit. On the third evening symptoms of iodoform poisoning appeared. Patient became very restless, later becoming delirious. She had severe pain in the abdomen, and iodine was found in the urine. There was no rash. On the fourth night the pulse, which had ranged from 104 to 110, rose suddenly to 140, and later to 160. On the seventh afternoon patient had a rigor, and complained of great pain in her back. The appearance of the patient at present would lead one to suppose that there was nothing wrong with her. The pulse is about 120, the temperature is subnormal, but she has suffered from incontinence of fæces. A culture was taken by Dr. Rowlette and showed no growth. The rarity of hypophrenic abscess following pneumonia, the accompaniment of pregnancy making it still more rare, are the reasons for the case being brought before you on this occasion.

Professor Smith said the first case opened up the very large

question as to operative interference during pregnancy. Formerly there was a superstition that they should not operate, but that had been quite altered, and there was not one of them who had not from time to time to do operations in pregnant patients. He had removed ovarian cysts and fibroid tumours from pregnant women, and in only one case in a number of such operations was pregnancy interrupted. The point was of considerable importance to the general practitioner. Personally he advised the removal of cyst as it might be a question of urgency at any moment owing to torsion of the pedicle, and they could not guarantee what might happen after delivery. As regards the time for operation, he would not operate until after the period had passed which corresponded to the usual time of the patient's menstruation.

Dr. Purefoy cited several cases in which he had operated without interrupting pregnancy.

Dr. FitzGibbon drew attention to a case in Mercer's Hospital the previous week of a young woman who had been operated on twelve weeks previously for gastric ulcer. She had come into Mercer's on account of vomiting, and a tumour low down in the pelvis transpired to be a four months' pregnancy, which had obviously begun some weeks before the operation, and had gone on without interruption.

Dr. Solomons, in reply, said the case of the cyst was up to full term. There had been two cases lately in the hospital about five months pregnant; one went to full term, and the other aborted after the operation.

Some Hints and Suggestions on the Teaching of Midwifery and Gynæcology.

Dr. Fred. W. Kidd read a paper on this subject. See page 235, *ante*.

Friday, February 25, 1910.

The President in the Chair.

Pyosalpinx.

Dr. E. Hastings Tweedy exhibited this specimen which had been removed from a lady who had been ill for twelve months as

a result of puerperal sepsis. She had been delivered in the country, and neither the doctor nor the nurse had been in time for the delivery. She got very ill, and was very septic. He explored the uterus, and took a culture, and found only a diplococcus like that of pneumonia. During labour the patient had a cough, and expectorated some muco-purulent discharge, but the doctor said there was no pneumonia present. He was again sent for in a fortnight, and found her worse than before. He performed Prior's operation. He packed the posterior fornix with iodoform gauze, and gave her diplococcus vaccine. She improved for a time, but after six weeks he was again sent for, and this time found a tumour about the size of a fist in the region of the right broad ligament. With great difficulty he opened into an abscess about Poupart's ligament. A culture once again grew only a diplococcus. After six months he saw her again, and she was a mere skeleton, and in a wretched state. The sinus was partially closed, and he opened it freely. Her temperature fell, and she got sufficiently well to be sent to the country. She came back with temperature 105° and palpitation of the heart. There was a large tube felt on the right side. The uterus was normal. When she menstruated the discharge flowed freely through the sinus. Again he sent her away, and again she got a temperature, and he decided a fortnight ago to do a radical operation. An old tubercular sinus that she had had as a child, and which was healed for many years, re-opened, and a quantity of very tubercular pus continuously exuded from it. He operated last week. The patient had been obviously losing flesh daily. The pulse was never under 110, and the evident involvement of the heart made him fear the operation. He injected the sinus with iodine, and cut round it and clamped it. Whilst doing so he unfortunately opened into it. He opened the abdomen and removed the tube. He plugged with iodoform gauze all round the raw surface. The patient's pulse was 130 when she went to bed. He waited until the third day to take out the plug. The plug came out with the greatest ease, but he felt convinced that her vitality was so low that no adhesions were made, for within two hours after the plug was taken out the pulse rose to 150 and 160, and she died acutely septic two days afterwards. He greatly feared that the incision he accidentally made in the sinus had been handled by the glove.

The bacteriological examination of the pus failed entirely to indicate the acutely septic nature of the sinus discharge.

Professor Alfred Smith said they could sympathise with Dr. Tweedy on the unfortunate result of so difficult a case. It was interesting to note that while septic troubles following parturition were generally regarded as bilateral, the case was exceptional in being unilateral. He had read that the best protective for surrounding an area where they were likely to open an abscess cavity was a rubber dam, which was used inside the abdomen, and could be adapted to any little crevice. He did not, however, think it would have made any difference in Dr. Tweedy's case.

Dr. Jellett said he thought Dr. Tweedy was right in attributing the fatal result to his own action; but he doubted if the infection came from the sinus: he thought it was just as likely to have come from the tissue around the abscess in the peritoneal cavity.

Pregnancy and Labour after Ventro-fixation of the Uterus (Hysteropexis Hypogastria).

Sir William Sinclair delivered an address upon the above subject. See page 252, *ante*.

Friday, April 1, 1910.

The President in the Chair.

Communication from Committee of Reference in regard to specimen of ovarian pregnancy reported by Dr. Tweedy at the meeting of December 3, 1909.

Dr. Rowlette said the specimen consists of an oval blood-clot, three inches long in diameter, to which is adherent a small piece of tissue, partially embedded in clot. The outer surface of the clot is laminated, and the coats peel off readily. On cutting through the specimen, the tissue is found to be tough, greyish-pink in colour. Between it and the clot is a bright yellow band, one inch long by three-eighth inch broad, obviously the wall of a ruptured corpus luteum in which the clot must have formed.

Microscopically.—The grey tissue is ovarian tissue, with a partial covering of peritoneum; the yellow band consists of

typical lutein cells; in the clot near the yellow band are numerous chorionic villi. No embryo was discovered.

The specimen is one of pregnancy, the ovum having developed in a Graafian follicle.

"We have examined the specimen described by Dr. Rowlette in the accompanying manuscript, and are of opinion that the case is one of ruptured ovarian gestation. The possibility of its having been tubal or tubo-ovarian is excluded by the fact that the tube was found to be normal and separate from the ovary at the operation. From the close relation of the fœtal structures to the lutein cells, it appears probable that the impregnation took place in a Graafian follicle, from which the corpus luteum subsequently developed.—H. C. Earl, A. C. O'Sullivan, H. Jellett."

(a) *Sarcoma of Body of Uterus.*

(b) *Carcinoma of Body of Uterus.*

Dr. Jellett exhibited specimens of the above by way of contrast. The patient in the first case was a girl, aged twenty. Her symptoms were frequent and heavy menstruation, and a considerable amount of leucorrhœa almost entirely of pure pus. Examination showed the uterus to be considerably enlarged and very nodular. He passed a Bozeman's catheter and washed away a considerable amount of pus situated at the upper part of the uterus. The diagnosis was apparently myoma, but he thought it might possibly be a case of sarcoma. As against the myoma was the age of the girl. The operation presented no particular features. He did a supravaginal hysterectomy, leaving a portion of the cervix. There was a tumour in the right broad ligament lying between the hilum of the ovary and the uterus. After removal the condition of the endometrium as shown in the specimen was plain to be seen. There was enormous proliferation apparently of mucous membrane all over the upper surface, and on the posterior wall a nodule almost like a myoma which had lost its blood supply. On examination it was proved to be sarcomatous. The uterine wall was extraordinarily thick. At first sight the specimen gave one the impression of recent parturition. A section through the uterine wall showed nothing but muscular tissue, and he did not know how to explain the thickening. The

patient made an excellent recovery. He had not removed the remainder of the cervix as the malignant growth was entirely separated from it by an inch and a half of tissue.

The second specimen was from a woman, aged about fifty-five or sixty years. She had gone into hospital complaining of profuse discharge. He found a large uterus, also nodular. He washed out the uterus and found great retention of pus. On scraping with a blunt curette he brought away a lot of sloughing growth. The scrapings were carcinomatous. On opening the abdomen he found the uterus comparatively free. There was very little extension of the growth in the neighbourhood of the cervix, and the fundus was entirely free. But on the left side there was a growth extending up into the pelvic glands behind the peritoneum. This he thought he was able to extirpate. On the other side there was a similar extension. The glands all along the aorta were enlarged. He removed them to the level of the second lumbar vertebra. He removed the uterus and a good deal of the vagina. The uterus lay low in the pelvis, and when he cut across the vagina he was within an inch of the vulva. The condition of the patient after the operation was fairly satisfactory, and he thought he would try to take away another gland higher up. He split the peritoneum further along the ureter, and found the remaining gland firmly adherent to the lower vena cava. The patient gradually got weaker, and died in a couple of days. The case was one of long standing, and he thought her heart muscle was weak. There were no obvious symptoms of sepsis.

Dr. Purefoy said the case of sarcoma was, in his experience, quite without parallel. He would watch for the subsequent history of the case, as he himself had had a patient aged sixty-five who died of secondary disease in the left lung a few months after he had removed a large fibrous uterus, which for thirty years had caused inconvenience only by its size. He did not remove the cervix. With regard to the second case, it was admitted that a proportion of the enlarged and hardened glands, the appearance of which suggested malignant disease, did not really contain it. Nevertheless, any of them who had such a case would, no doubt, endeavour to remove the glands as completely as possible. In the case of an aged patient, they had to consider whether they were warranted in carrying out a prolonged

operation for the complete removal of all the enlarged glands they could feel.

Dr. Jellett, remarked that Sir William Sinclair, on his recent visit, had said to him that he was in the habit of telling his class that cancer of the body of the uterus occurred only in comparatively well-to-do cases, and cancer of the cervix in very poor patients. Sir William's idea was that the only way to prevent cancer of the cervix was by improving the conditions under which the patients lived. In his (the speaker's) experience the rule did not apply in Ireland.

Dr. Holmes said that, looking back on the cases in the Rotunda, the number in the extern dispensary of cervical cancer seemed to him to be more numerous than the house cases. Neglect of discharges might set up disease, but the upper classes, if anything went wrong, generally had the matter seen to.

Dr. Purefoy said his experience did not correspond with Sir William Sinclair's. He would like to know if, from inspection of the sections, it was possible to determine whether the sarcoma had occurred in the connective tissue or muscular cells.

Dr. Jellett said that there was a doubt in the mind of Prof. O'Sullivan, who reported on the specimen as to its origin in the ovary. Prof. O'Sullivan's report is as follows :—

"The uterus was large, the body being four inches in length and the wall a little over an inch thick at the thickest part. The upper two-thirds of the cavity were filled by an irregular growth, with numerous polypoid excrescences projecting into the lumen. The surface of the growth was acutely inflamed and partially necrosed, and covered in places with a thick layer of pus, which contained a variety of organisms—bacilli and cocci. The growth could be seen to infiltrate the posterior wall of the uterus, and secondary nodules could be felt in the upper and posterior part of the wall. The ligament of the ovary was also infiltrated, and the ovary itself was converted into a mass, partly nodular and partly cystic. Under the microscope the tumour showed a very cellular structure, spindle and large round cells with numerous nuclear figures. The solid tumours in the ovary had a similar structure. A large nodule lay in the upper uterine wall, which was of a dull grey colour on section, and proved to be of similar structure."

Suppurating Ovarian Cyst with Tubercular Pyosalpinx.

Dr. FitzGibbon exhibited this specimen. A girl, B. N., aged nineteen, had four years ago tubercular disease of a finger, which was amputated. Her menses, which had always been regular from the time they commenced, ceased in November, 1909, and shortly after that she developed a cough, and had several attacks of vomiting, accompanied by pain in the abdomen. She was admitted to Mercer's Hospital on January 3, 1910, and the speaker first saw her about the end of January. She then had an irregular temperature, varying between 99° and 101°, and pulse from 95 to 110. She had ceased to vomit, but complained of acute pain in the abdomen, and was very constipated. A tumour could be felt extending to one and a half inches from the umbilicus. On vaginal examination the pelvis was found to be filled up by a cystic tumour, which was fixed, and which pushed the uterus upwards and forwards. The left appendages could be felt apparently normal, but the tumour prevented any examination of the right side of the pelvis. The patient was kept in bed and the bowels were freely acted upon, and then the temperature improved, but the pain continued to be severe. On March 1st the abdomen was opened, and an ovarian cyst was found springing from the right side and firmly adherent all over the back of the pelvis. The intestines were completely shut off by the omentum. In freeing the cyst from the pelvis it ruptured, and discharged about two pints of purulent contents. The left tube was greatly thickened and adherent, also containing pus. By working down the left side of the uterus, and then dividing through the vagina the right appendages were shelled out. The left tube which was removed was perfectly normal, and the left ovary was retained in situ, as it seemed quite healthy. The pelvis was drained through the vagina with iodoform gauze passed from above down, and only iodine catgut was used throughout. Cultures were made from the pus in the tube immediately after the operation with a view to making an autogenous vaccine if the case developed into one of acute sepsis, as was anticipated from the amount of pus which unavoidably got about the pelvis; also saline enemata, two pints, were given every two hours for two days. The temperature rose after the operation to 102°-103°, and the pulse to 130 to

140, and remained fairly steady for the first four days, when they began to improve, and have since remained from 99° to 101° and 100 to 120. The lower inch of the abdominal wound opened and discharged a foul-smelling pus for about ten days, and is now granulating, while the pus from the vagina went through the same stages. The patient looks better, is free from pain, but continues the high temperature and pulse. The report received on the specimen was that the cultures contained no pyogenic organism, and except a bacillus which resembled *B. coli* the pus did not show any other pyogenic organisms. Sections made from the wall of the ovarian cyst proved to be tubercular, while the left tube was normal. The ovarian tumour showed a large single cavity, strongly suggestive of an abscess, and in no way resembling an ovarian cystoma.

Dr. Purefoy said he understood there was no evidence of gross uterine disease, and the removal of the uterus did not commend itself to him as the best proceeding under the circumstance. It increased the area in which infection might take place, and it was a serious loss to a patient at the age of the case. He thought an adequate opening might have been found in Douglas's space.

Dr. Rowlette said the association of tuberculosis and cystic growth in the same ovary was, he thought, very rare, though one saw frequently tuberculosis of the tube associated with a certain kind of cystic growth of the ovary. It was indeed rare for any tumour to become infected by tuberculosis. He thought the cyst was really an abscess cavity.

With regard to the President's specimen, it seemed to him to be an advanced case of sarcoma starting in the endometrium. He should say the nodule was sarcoma from the beginning. The fact that the uterus was so large showed, he thought, a good deal of resistance on the part of the patient, and that the growth was not very rapid. The fibrosis must have been a reaction to the growth of the tumour, which would be encapsuled to a certain extent.

Dr. Jellett, speaking on Dr. FitzGibbon's specimen said he had never before heard of a specimen of tuberculosis of an ovarian cyst, and the first thing to know was the nature of the cyst. If it was purely an abscess starting in the ovary, then the tubercular infection would not be quite so unusual. But, if it

was a proliferating cyst of the ovary, he did not know that he had ever heard of such a condition being recorded before, and he thought Dr. FitzGibbon should be asked to give them some further information at a later date.

Dr. FitzGibbon, in reply, said the section showed typical tubercular infection, but he did not think any one could say definitely that it was from a proliferating ovarian cyst, in fact the inner surface of the section did not show anything to suggest ovarian cystoma, but strongly resembled the lining of an abscess cavity. He thought the case, now that he had obtained and seen a section of the tumour, was one of tubercular ovarian abscess. In reply to Dr. Purefoy, he said the condition of the patient appeared so bad, and such a quantity of pus had escaped about the pelvis, that he thought the only chance was to obtain absolutely free drainage, and that this would be best by removal of the uterus, even then he hardly expected the patient to survive.

Our Responsibilities in the Prevention of Inherited Syphilis.

Dr. Sheill read a paper on above. See page 264, *ante*.

Depressed fracture of the Skull treated by Bullet-forceps.

Dr. Solomons showed a specimen of a fractured skull which had been raised by means of one blade of an American bullet forceps. He said that the case occurred in a quartipara, who gave a history of her previous babies having had "dents" in their heads. Her pelvic measurements were:—interspinous, 27 cm.; intercostal, $28\frac{1}{2}$ cm.; external conjugate, 17 cm.; conjugata vera, $\frac{1}{5}$ cm., transverse of inlet, $13\frac{3}{10}$ cm. Prolapse of the cord occurred when the membranes ruptured, and version was performed and the child delivered. When born it was found that there was a depressed fracture of the right parietal bone. This was raised as follows:—The point of a sharp bullet forceps was placed in the centre of the depression, the forceps was turned through a right angle, and then raised; it was easily to be seen in the specimen that the cure of the fracture was complete. This method, which was first performed by Dr. Tweedy, had been carried on in the Rotunda Hospital with uniform success. In this case the infant became jaundiced soon after birth, and died on the third day.

There was no paralysis. A *post-mortem* examination revealed nothing except the mark of the bullet forceps in the brain to the extent of possibly one-eighth of an inch, and evidently this had nothing whatever to do with the death of the child.

Dr. Neill said he had seen a deep dent in a child which was born alive, and spontaneously recovered without interference.

Dr. Jellett asked if it was the practice to lift all dents with bullet forceps.

Dr. Pearson said the skull of a new-born child was a very pliable structure, and might not require any treatment to elevate a fracture. From a surgical point of view the objection which he saw was the possibility of injuring a meningeal vessel.

Dr. Holmes thought any noticeable depression ought to be raised. He had used the method several times, and the children did very well.

Dr. FitzGibbon said there was a hæmatoma between the dura mater and the parietal bone, not very extensive, and he asked if the bullet forceps had punctured the dura mater.

Dr. Solomons, in reply, said all depressions of any import were raised. There was a puncture in the dura mater when the specimen was fresh.

SECTION OF PATHOLOGY.

CONCERNING TRACHOMA.

By ARTHUR H. BENSON, F.R.C.S.I.;
Surgeon to the Royal Victoria Eye and Ear Hospital, Dublin.

[Delivered in the Section of Pathology, November 12, 1909.]

My first duty is to thank you for the honour you have done me in electing me President of this Section, and I do so with all sincerity. My next duty is to thank Professor White, on behalf of the Section, for his long and faithful services as secretary, and to greet his successor, Dr. Boxwell.

It is a very usual thing, in opening a new session, for the President to review the work done during the previous session; but in the present case such a recapitulation would seem to me to be profitless. I prefer, rather, to call your attention to one subject of the first importance to all humanity, and to the inhabitants of this country in particular, in the hope that by your efforts a serious blank in our knowledge may be filled.

Trachoma, or granular ophthalmia, is one of the greatest curses that weigh down and oppress the poor in this distressful country. It is the plague of all our workhouses, industrial schools, and eye hospitals. It is the "opprobrium ophthalmicum"; for though, like the poor, whom it principally attacks, it is always with us, yet we have no certain knowledge as to its true nature or its ultimate cause.

I trust that some of those members of the Section who have energy, and enterprise, and time, and opportunity,

will undertake original investigations into the essential cause of the condition which, year by year, is responsible for the blinding of thousands and the partial incapacitating of tens of thousands of our fellow-countrymen.

Whoever discovers the actual essential cause of trachoma will thereby erect for himself a monument more lasting than brass, and will have earned the gratitude, not only of a nation, but of the whole world.

Much has already been done to prepare the way. A few years ago every form of conjunctivitis was described and catalogued solely in accordance with its clinical appearances and characteristics. Now most of these forms are identified bacteriologically, and their specific cause is known with certainty.

Acute catarrhal conjunctivitis is known to be caused by the slender bacillus of Koch and Weeks. The Morax-Axenfeld diplo-bacillus produces a characteristic form of blepharo-conjunctivitis. The pneumococcus produces a less characteristic form of conjunctivitis, with pinkish œdema of lid margins, and often rapid subsidence of the symptoms, reminding one of the "crisis" in acute pneumonia. Löffler's diphtheria bacillus produces its characteristic membranous conjunctivitis. The gonococcus is responsible for gonorrhœal ophthalmia, and many cases of ophthalmia neonatorum. The streptococcus, bacterium coli, and tubercle bacillus each produces forms of conjunctivitis that are definite clinical entities, and as the cause is recognisable, so the line of treatment emerges from the twilight of empiricism into the full day of scientific certainty. But trachoma, which draws its victims from all periods of life, and with bulldog tenacity holds them in its grip, as yet owns to no specific cause, yields to no specific toxin. We still treat it much as it was treated fifty years ago (or, for the matter of that,

two thousand years ago), and with a most humiliating want of success. All the evidence points to its being micro-organic in origin, but the special causative germ — this "Scarlet Pimpernel" of bacteriology — still eludes the vigilant search of microscopes in every land.

Every few years our hopes are raised by the publication of a full description of the "notorious criminal," with portraits of his development, from youth to age, but, so far, in every case he has been able to prove an *alibi*, and has left the court without a stain upon his character.

Just at present the trial of the most recent "suspect" is in full swing. Through the kindness of Dr. Mooney, I am enabled to show you not only his portrait, but the individual himself under one of the microscopes. The so-called "Trachoma corpuscle"[a] of Greeff. Professor Greeff sent him to Sir Henry Swanzy, who lent him to Dr. Mooney, who brought him here for us to see.

Later on we shall have more to say on the subject.

Trachoma is known in practically all countries. It is a scourge in many, and its presence has been recognised from the remotest ages, but it was the devastating epidemics of the latter years of the eighteenth century, and the beginning of the nineteenth, that first riveted upon it the attention of the medical profession in Europe.

In 1798 Napoleon landed an army of 32,000 men in Egypt. A very large number of these were attacked by a violent inflammation of the conjunctiva, to which the name "Egyptian ophthalmia" was applied. On their return to Europe they brought the disease with them, and in the period of the Napoleonic wars, when all the armies of Europe were repeatedly in contact, the disease

[a] Provazek calls them "Clamydozoa" (Manteltiere, *i.e.*, organisms with mantels). Many writers call them "Provazek bodies."

spread with such appalling rapidity that a few years later we find it epidemic in every army in Europe. In the English army, during the year 1818, there were more than 5,000 men on the invalid list, blind from trachoma. In the Prussian army, from 1813 to 1817, over 20,000 men were attacked with this disease. In the Russian army, from 1816 to 1838, 76,811 men were suffering from trachoma. In Belgium, in 1840, one out of every five soldiers was afflicted with trachoma.

These soldiers, invalided home, carried the disease to their families, and spread it through every quarter of their country. Hence trachoma became epidemic in almost every state of Europe. At this stage it became usual to describe it as "military ophthalmia."

Among the civil population trachoma finds a soil suitable for its development wherever many persons are crowded together with imperfect hygienic surroundings. Hence, when in 1841 workhouses became established in Ireland, they formed excellent culture beds and disseminating depôts for the disease, and, in spite of the improved hygienic arrangements, trachoma still flourishes in every union workhouse throughout the land.

It is a remarkable thing that whereas trachoma is a national calamity in Ireland, it is an extremely rare disease in England, and would most assuredly become extinct there but for the influx of infected aliens.

England, as has been said, is a trachoma filter-bed for the United States, and Ireland suffers in the same manner, but the better hygienic conditions prevailing in England have saved it from our fate.

Trachoma is a disease venerable by its antiquity. It is believed to be the disease alluded to in the oldest book on medicine that is known, the Ebers Papyrus, written in Egypt about three thousand four hundred years ago—

i.e., about a thousand years before Hippocrates. Celsus describes the disease, not by name, but in indisputable language. It was well-known among both the ancient Greeks and Romans, as Boldt mentions in his exhaustive work on "Trachoma" (from which, as also from Fuchs and Axenfeld, I have borrowed freely).

It is alluded to in the comedies of Aristophanes, and in a certain Greek play those desirous of avoiding service as marines are represented as pleading ophthalmia as an excuse.

How modern these old Greeks were!

It is stated that Meshullam ben Menachem, in 1481, and Obaja, in 1488, are the first to mention that eye disease was then, as it is still, almost universal in Egypt; but how long before this the disease had become pandemic it is difficult to say.

Trachoma, with us, shows itself under two rather different aspects:—

(1) *Papillary trachoma*, characterised by an increase in size of the surface of the hypertrophic conjunctiva, so that it is thrown into folds with deep clefts between: the connective tissue forming the papilla is stuffed full of round cells; and

(2) *Follicular trachoma*, characterised by the presence of the true trachoma bodies, which are rounded accumulations of cells, between which a very delicate connective tissue framework can be made out. The cells in the marginal portions of the granulations are lymphocytes; in its anterior parts are predominantly uninuclear leucocytes (epithelioid cells), between which lie a few particularly large cells (phagocytes), which contain in their protoplasm small, deeply-staining corpuscles (Fuchs).

There are certain points which may be worth bearing in mind by anyone about to undertake an investigation

into the cause, the intimate specific cause, of trachoma Amongst the lower animals monkeys are said to be the only ones affected with trachoma. The conjunctiva and lacrimal sac are the only mucous surfaces affected with the trachomatous type of inflammation.

Trachoma cases are nearly always complicated by the presence of some one or more of the well-known pathogenic organisms, which must be regarded as a complication, and not the cause of the condition, a mixed infection being almost the rule, the most important complicating organisms being the Koch-Weeks bacillus and the gonococcus.

The history of the earlier epidemics, and the frequent presence of the gonococcus, has led some to hold that trachoma arose originally from the action of the gonococcus. Indeed so lately as 1905, Bishop Harman writes:—

"The conclusion I am driven to, after examining all the evidence at my disposal, is that, so far as microbic influences can be determined, the trachoma of our time is probably the result of an inoculation of the conjunctiva with the Micrococcus gonorrhœæ of an attenuated virulence, the effects of the inoculation being so mild that they are not noticed; consequently, in England we never obtain definite evidence of the organism, the case being seen too late."

Trachoma is essentially a chronic disease. Acute trachoma is a disease which begins acutely with profuse secretion, and in the majority of cases is not a pure trachoma, but a mixed infection with an acute catarrh.

Perhaps the most perplexing fact in connection with the subject of the contagiousness of trachoma is the unilaterality which is now and then observed. I have several times found a typical trachoma of several years' standing in one eye, whilst in the other eye of the same

ndividual no trace of disease, past or present, could be een. No satisfactory theory has, so far, been put for- ard to explain this anomaly. It is impossible to sup- ose that direct local infection had not taken place luring those years, and such explanations as "local mmunity" are mere words, and do not help us.

The infectiveness of trachoma is greatly increased luring the acute exacerbations caused by the presence of ther organisms, such as the Koch-Weeks bacillus and he gonococcus, the diplo-bacillus of Morax-Axenfeld, or he pneumococcus.

Simple chronic granular ophthalmia possesses but a ow degree of infectiveness, but predisposes the con- unctiva to the attacks of other micro-organisms, whose ctivities at once raise the infectiveness of the trachoma irus. Drying of the conjunctival secretion seems en- irely to destroy the infectiveness of the trachoma virus. No life period is proof against trachoma. Acquired mmunity cannot be demonstrated. The influence of ersonal disposition cannot be denied. The influence of ace predisposition is doubtful. The influence of en- ironment is all-important.

Trachoma has never been produced by inoculation with ure cultures of any known micro-organism, though it as from time to time been stated to be caused by—

(1) The gonococcus, as mentioned before.

(2) The Koch-Weeks bacillus (Gromakowski).

(3) Micro-organisms, designated "trachoma cocci," ave been found by Michel, Sattler, and many others, ut Axenfeld says they are not constant, and are not the ause of the condition.

(4) Bacillus Xerosis (Shongolowicz).

(5) "Microsporon trachomatosum," a fungus which oiczewski thought he found.

(6) Ultra-microscopical bodies are described by Raehlmann and Santucci, but their causative power requires proof.

(7) L. Müller has found a small bacillus like the influenza bacillus, but it is not constant, and is not the cause of trachoma.

Everything is in favour of trachoma being a specific infectious disease, the cause of which is not yet definitely determined; but, as I mentioned before, there is now another prisoner in the dock—the organism discovered almost simultaneously by Professor Greeff in Germany, and by Halberstädter and Provazek in Java, and verified by Mijaschita in Japan, and by others in different countries.

At the Ophthalmological Congress in Oxford, in July last, and subsequently in Belfast, Professor Greeff demonstrated the organism which he regards as the cause of trachoma. He says—"The bodies I found were very regular, round, cellular inclusions, which were much smaller than the smallest known coccus. They stain intensively, sometimes violet, sometimes reddish or blue, with Giemsa, diluted with aniline stains, and not at all with Gram. They are surrounded by a distinct, clear zone. With the strongest powers of the microscope, one observes that they are not quite round, but a little oval, like bacteria with rounded extremities. Finally, they are isolated, or grouped in pairs or in masses. If intracellular they lie close to the nucleus. We found the formations in the epithelial cells, in the discharge, in the pressed-out follicles, and, indeed, everywhere they were looked for."

The bodies can also be seen in sections "in the subepithelial tissue, in the lymph spaces beneath it, in the cells of the follicles (the lymphoid and the so-called Leber's cells), and between the cells."

Greeff states that these bodies are a constant characteristic of trachoma, as he has found them in every recent case of trachoma (not under treatment), and, further, that he failed to find them in any other form of conjunctivitis.

Caustic applied to the conjunctiva makes them vanish for the time, but they recur. These bodies invade the cell, increase in number, and congregate in a mass close to the nucleus, multiply till they fill the cell, and finally burst free into the tissues.

"It would as yet," he says, "be premature to attempt to assign the trachoma bodies their place in the zoological system. They are certainly not bacteria; on the contrary, they are more closely allied to the protozoa." [a]

In these disjointed remarks I have brought forward nothing original, I have stated nothing new. My object has been to attempt very briefly to recapitulate some of the results of the last twenty years' work on this subject, in the hope that it may serve to stimulate some of the younger men here to undertake the task of verification, and by original work to throw light upon these dark places.

Are these organisms really the cause of trachoma? That is the great question of the hour. It may be easy to demonstrate their specific nature, but their parasitic and causative nature is quite another question.

We have so often been disappointed in the past that it behoves us to accept with caution all statements, and test for ourselves the facts. At present our only verdict can be, "Not proven."

I know the subject is difficult, but if it were not difficult neither would it be interesting. I know that it is not easy to get the time and the energy to carry on any extra

[a] A cell with a mass of these in it was on view under the microscope.

work, for, as the great Samuel Johnson said, "What we think of importance we wish to do well; to do anything well requires time, and what requires time commonly finds us too idle or too busy to undertake it."

Through the labours of your predecessors, as I have tried to show, many solid facts have been established; much of the mist and obscurity that surrounded the subject has been cleared away. But there is plenty of original work still to be done, for, even supposing that the organism now shown under the microscope is the long-sought microbe, that is only the first step. Its life history has to be worked out, its origin and its metamorphoses have to be investigated, and its likes and dislikes observed. Not till these have been done can any advantage be obtained from the primary discovery.

The material for such investigations is at your hand. I can give anyone who wants to work as many cases of trachoma as he wishes. Our eye hospitals are full of them, our workhouses are full of them, our blind asylums are full of them, the whole country is full of them—there is no lack of material. Much has been done, but much remains to be done, and this is the place to do it.

In conclusion I will tell you a story.

Two mice in a pantry were foraging for food. One of them slipped into a jug of cream, and was like to drown. "Keep kicking," cried his friend on the shelf. "But I can't swim," said the one in the jug. "Never mind," said his friend on the shelf, "keep kicking, keep kicking." "What good will that do?" said the half-drowned one in the jug. "Never mind, keep kicking."

In the morning, when the pantry was opened, Mr. Mouse in the jug was found sitting comfortably up on a pat of butter! My advice to you is "Keep kicking."

PIROPLASMOSIS AND EXPERIMENTAL PIROPLASMOSIS.

By A. E. METTAM, B.Sc., M.R.C.V.S., M.R.I.A.;
Principal of the Royal Veterinary College of Ireland;
Professor of Pathology and Bacteriology in the College.

[Read in the Section of Pathology, March 4, 1910.]

Piroplasmosis is an infectious disease due to the presence in the red blood corpuscles of an animal parasite—a piroplasm. The Piroplasmata are protozoa of the group Sporozoa, belonging to the sub-division Hæmosporidia. The complete life-history is not yet known. The definitive host is a tick, and the disease is transmitted by an infected tick. The life history in the tick is not understood, but probably the sexual stage of the parasite's existence occurs in the tick—in the mammal the schizogonous stage alone is passed just as in malaria in man. In piroplasmosis, however, we do not recognise either macrogametes or microgametes—the phases of development observed are those associated with the formation of merozoites or spores. The parasite in the corpuscles divides entirely into two merozoites or spores, which sooner or later leave the corpuscle, or the corpuscle dissolves and sets them free, and the free parasites then invade other corpuscles to again go through the same process.

Piroplasmosis is observed among the domestic animals, in the horse, ox, sheep, and dog, and probably in the ox there are several different parasites capable of producing disease varying somewhat in severity and in symptoms. It is a remarkable fact that the Piroplasmata pathogenic for one

species of animal are not capable of producing disease in another species—that, in other words, Piroplasmata of the ox are not capable of producing disease in the horse, sheep, and dog, and the organisms found in these animals are incapable of infecting the ox. The usual laboratory animals cannot be utilised, they are not infected, and consequently in research only animals of the same species can be utilised in any investigation made into the piroplasmosis of any particular species. Many of the piroplasmoses may be produced by inoculation of infective blood, and after a period of incubation, varying with different Piroplasmata, infection is established, and in some cases hæmoglobinuria appears.

Hæmoglobinuria is a common symptom in native piroplasmosis as it is in Texas fever and allied infections, but not in all. There are certain piroplasmoses where hæmoglobinuria is absent and jaundice is pronounced. In equine and canine piroplasmosis the icterus is intense, and the former infection is called biliary fever, the latter malignant jaundice. In dogs, however, hæmoglobinuria is not uncommon, and the first case recorded in France had attention first directed to it by the hæmoglobinuria shown by the patient. Animals that have recovered from an attack of piroplasmosis are immune, though the immunity may break down either by a second virulent infection or by some intercurrent infection. Moreover, the blood of such immune animals is infective though the parasites cannot be found by the most careful and painstaking search of blood films. Injection of the blood into susceptible animals may be successful in setting up infection. Ticks feeding upon such immune animals become infective, and are capable of infecting susceptible animals upon which they feed. Immune animals, therefore, pastured with non-immunes may be the source of an infection through the medium of ticks.

What I have just said applies to most forms of piroplasmosis, but there is the infection known as East Coast Fever, which differs in a remarkable fashion from ordinary Red Water. The organism is a small parasite, sometimes bacillary in form—the so-called *Piroplasma parvum*. The infection is extremely fatal—more than 90 per cent of infected animals die, and it threatened to annihilate the herds of South and East Africa. In pure infections by the *Piroplasma parvum* there is no hæmoglobinuria; it is not transmitted by even massive inoculations of blood containing parasites in practically every corpuscle. Animals that recover are immune, but the organism is not present in the blood. Ticks sucking the blood of such immune animals do not transmit the disease to non-immune animals. Transplantation of portions of the spleen into the abdominal cavity of cattle is said to produce East Coast Fever. The disease differs, therefore, very markedly from ordinary piroplasmosis. There is some reason to believe that the organism is not a piroplasm, and recently a new class has been created for it, and it has been named the Theileria parva, after Dr. Theiler, the Veterinary Bacteriologist in the Transvaal.

In some cases of East Coast Fever hæmoglobinuria was observed, but it was found that there was also an infection by the *Piroplasma bigeminum*, and to the ravages of this parasite the "red water" was due, but in pure uncomplicated infections there is no "red water." This may also be the explanation of the success attending injections of blood from cases of tropical piroplasmosis, which Theiler believes is due to the same cause as East Coast Fever. In South Africa, also, there is another piroplasm—the *P. mutans*—which may complicate cases of ordinary piroplasmosis as well as East Coast Fever. An interesting point has been discovered which is of value for diagnostic

purposes of East Coast Fever, and that is that splenic punctures afford material upon which a diagnosis may be based. In smears made from the spleen and stained by any modification of the Romanowsky stain, preferably by the Giemsa solution, round bodies the size of a lymphocyte or larger, of a light blue colour, containing red granules,. are to be found. These bodies, known as Koch's bodies or granules, are claimed to be specific for East Coast Fever. It is not known what these are, but they are not unlike erythrocytes showing polychromasia, though the granules observed react differently from the granules found in red blood corpuscles in other piroplasmoses.

The Organism.—The Piroplasmata are intra-corpuscular parasites, and may be demonstrated in films fixed and stained in the usual way. It is advisable not to use heat in any stage of the staining process. A thin film should be made and rapidly dried in the air by waving it about. The film is then fixed by methylic alcohol, though absolute alcohol or even methylated spirit will do. The spirit, after five minutes, is washed off and Giemsa's solution—1-10—applied for ten minutes or a quarter of an hour. The stain is run off, the film well washed in water, the water blown off, and the film dried by waving in the air. When dry it may be examined at once by placing a drop of cedar wood oil upon the film and turning on the oil immersion lens. Leishman's stain fixes and stains at the same time, and it is a most useful reagent. After the alcoholic solution of the dye has acted for a minute or two double the amount of water is added to the stain on the slide and mixed, allowed to act for ten minutes or longer, washed off with water under the tap or from a wash bottle, the remnant water blown off, and the film rapidly

dried in the air. No heat should be used in any stage of the staining process. In my hands neither Wright's nor Jenner's stain is nearly so good as either Leishman or Giemsa.

A variable number of corpuscles is infected. Sometimes it is necessary to search the edge of the film, where they may be found even if few in number. In severe cases at the height of the fever many corpuscles may be found infected and containing parasites in the ring or ink form or as pear-shaped organisms joined by one extremity or free. The *Piroplasma bigeminum* is 3.5μ long by 1.2μ thick at broadest part. In some cases several may be found in a single corpuscle, eight or more in canine piroplasmosis, four like a cross or rosette in equine piroplasmosis. In other corpuscles modifications of the ring-shaped or pear-shaped organisms may be encountered. The organism is stained (by Leishman's stain) blue, and at one place is a dot-like object, the nucleus which stains ruby red. The red granule is at the broad end of the pear, but Lüke describes another much smaller at the pointed narrow end. The blue stain is deeper tinted on the edge of the parasite, the centre being paler in tint or even unstained. The Giemsa stain renders the parasites more conspicuous as it colours them more deeply. Many free parasites may be found both ring-shaped and pear-shaped; the pear-shaped forms may still be joined or free. The parasites may leave the corpuscle, or more frequently the corpuscle degenerates and disappears, or may be seen as a ghost, the reaction to the eosin of the dye having gone. The free parasites seek a new corpuscle, enter it, and begin the course of schizogony. The pear-shaped parasite on entering a corpuscle becomes rounded and amœboid. Two small processes project from the para-

site, each receiving a moiety of the nucleus. The processes increase in size at the expense of the ring-shaped parasite, and soon it appears as if divided into two, each with its ruby-red stained nucleus. The young spores or merozoites remain attached by an attenuated extremity for some time. They look like twin pears on a common stalk, but later this union disappears, and two free pear-shaped organisms are found. This appearance of twin pears suggested the name of the parasite—the *Piroplasma bigeminum*. The appearances in piroplasmosis generally are as I have described, but there may of course be variations—irregular forms as well as vagaries induced by multiple infections.

When infection is passing off it is not unusual to find red blood corpuscles containing numerous fine granules staining with the basic dye, and these corpuscles and their contents are sufficient for diagnostic purposes. These granules are due to fragmentation of the parasite, or to liberation of granular matter from the parasite. I have seen cells containing what appears to be a degenerating parasite having numerous blue granules scattered through the body of the cell, and directly traceable to the parasite.

Some observers have described a flagellum as occasionally being present attached to the parasite. The process is not a flagellum, but, as Minchin suggests, a pseudopodium. It stains, like the body of the parasite, blue, and terminates in a knob or bulbous structure. Parasites showing these pseudopodia are not common, and those I have seen are within the corpuscles.

There is no pigment formed by the Piroplasmata, and in this they differ altogether from the organisms producing malaria.

The presence of the parasites in the red corpuscles

auses great corpuscular destruction, and the liberated æmoglobin is excreted through the kidneys, colouring he urine. The colour may be merely dark brown to red, lark red to black, even porter like. There is also bundance of albumen in the urine. This albumen is robably the result of a lesion of the glomeruli of the idney.

The after-effects of piroplasmosis are those of anæmia, nore or less profound. There is sometimes poikilocytosis nd anisocytosis. The latter condition is particularly triking and of value in diagnosis, as it persists for some ime after all the parasites have disappeared from the lood.

The Piroplasmata, though generally resembling each ther in appearance, are not all of the same size, and his difference in size is inherent to the parasite and not he result of artefacts. This is proved by the fact that *P. bigeminum* and *P. mutans* may be identified in the same nimal.

There is little doubt that the common cause of piro-lasmosis in Ireland is different from the ordinary lassical *P. bigeminum*, and I am of opinion that here are at least two different forms of piroplasmata ffecting cattle in Ireland. I have seen organisms in-listinguishable from the typical *P. bigeminum* and thers that are much smaller and differ considerably n appearance from the bigeminum. In my experience, lso, this second parasite is much more common than the ormer parasite.

I would direct the attention of those present to ex-mine the slides from native cases of piroplasmosis and to ompare the parasites observed in the corpuscles with hose of the experimental cases.

Piroplasmosis is not only common in tropical and

sub-tropical countries, butit is very prevalent in temperate climates all over the world. It is the so-called Texas Fever of America, and Red Water wherever it is met with. Naturally, the disease is transmitted by the tick, no matter whether it is the piroplasmosis of the horse, ox, sheep, or dog. The virus may pass from the mother tick through her eggs to the young ticks developing from the eggs. These young ticks may transmit the disease during their larval or nymphal stage, or only when they become adult. The virus may be transmitted by the young tick, which has been infected as a larva, in the nymphal stage, or in the adult stage when it has been infected as a nymph. The information we have as to the *rôle* of the tick as a transmitter of the virus of piroplasmosis reads like a romance, and is a monument to the incredible industry and application of the workers in the field. There still remains, however, a hiatus to be filled in by the discovery of the life of the parasite in the definitive host and the condition of the parasite as it leaves the tick to enter the intermediate host. The fact that the virus is present in the eggs produced by the mother tick that has fed on infective blood is particularly interesting.

Experimental Piroplasmosis.—In South Africa, and also in England, piroplasmosis has been produced by allowing infected ticks to suck the blood of non-immune cattle, and similarly canine piroplasmosis of South African origin has been produced at Cambridge by N ttall and Graham Smith in dogs. Piroplasmosis has also been produced in cattle, sheep, dogs, and horses by inoculation of virulent blood into susceptible animals. But to confine ourselves to bovine piroplasmosis. All animals are not equally susceptible. Young animals—calves—possess at birth a high degree of immunity, and

alves running with their mothers live and thrive though ne older animals are suffering and dying from the isease. Both young and old have been exposed to the ame infection and infecting agent, yet the young ones urvive; the older are infected and may die. An nimal born upon a farm where piroplasmosis is endemic, ossessing a natural immunity, as all calves have, has is natural and inborn immunity strengthened by the epeated inoculations of infected ticks. The immunity s thus rendered permanent—an immunity which would ave disappeared if it had not been established by re-eated infections. It is the repeated attentions of the icks which keep up the immunity of animals upon an nfected field or farm. If an animal—though born with natural immunity, which sooner or later if not rein-orced disappears—lives upon a farm free from "red ater," and then is changed to a place where "red water" s prevalent, or where the ticks are infected, then, when t is attacked by the infected ticks, it sooner or later evelops "red water." It has lost the immunity it had, nd succumbs to attack. It may, and frequently does, ecover, but the blood is infective; it may set up the isease when inoculated into a susceptible animal, and he ticks drawing the blood become infected. The fact hat animals are born with an immunity, and that this mmunity may be prolonged indefinitely by mild infec-ions, may be taken advantage of to immunise animals gainst the disease, although such a procedure would ecessarily require all animals to be so protected because f the ticks. Animals immunised by injections of blood ay have the immunity broken down in the field, due it ay be to some other infection, or, perhaps, to infection y some other piroplasm than the one it is immunised gainst. The Africander ox is immune to "red water" of

South Africa. Texas cattle, presumably immune to Texas Fever, when imported to South Africa may develop piroplasmosis. The same occurred with South American cattle. It appears that one form of piroplasmosis does not give immunity against invasion by a parasite different from that producing the original infection. That this is not always the case is possible.

When blood is injected into a susceptible animal after the lapse of an incubative period, which may be about six or seven days for *P. bigeminum* to twenty-one days for *P. mutans*, the temperature begins to rise. Soon afterwards the parasites make their appearance in the blood, and if the piroplasmosis is associated with hæmoglobinuria the urine becomes darker in colour and contains albumen. The parasites increase in numbers, so that a large percentage of corpuscles contain parasites. Many red corpuscles are destroyed, the urine becomes still darker until it may be the colour of porter. The infection may be so severe that death results, or the infection passes off, the temperature falls, the parasites become fewer in the blood, the urine returns to its normal colour and condition. The animal has lost some condition, and may show this loss for some time. There are indications of anæmia with changes in the red blood corpuscles as previously noticed.

With respect to the piroplasmosis in this country I have on several occasions injected large quantities of blood into animals without any result. I have used not only blood obtained on the farm and conveyed to the laboratory, but I have got the patient to the laboratory and without delay injected the undefibrinated blood into the veins of cattle. I have used also emulsions of the spleen where numerous piroplasmata could be readily demonstrated, but without success. In some instances a

w piroplasmata could be found in the inoculated cattle few days after, but there was no temperature reaction, "red water." An example may suffice. A cow naturally fected was obtained, and after examination of her ood, which contained numerous piroplasms, her jugular in was tapped, and 100 c.c. of blood were withdrawn. c.c. of the blood were injected subcutaneously into a ifer, and 20 c.c. into a bullock. The animals had been the house for seven months. 20 c.c. of blood were in-cted into the jugular vein of an aged cow. The animals ere kept under close supervision, and the temperature as taken twice a day from the date of inoculation—May th, 1909, to July 13th in the case of the heifer and w. The bullock was killed on July 8th, as it was ffering from Johne's disease experimentally produced. he highest temperature registered for the cow was 3.4° on the evening of June 13th, for the heifer 103.2° June 8th, for the bullock 103.6° on June 8th. The mperature of the cow on the evening of May 28th was 1.8°, of the heifer 101.7°, of the bullock 103.0°. I do t consider there was any temperature reaction in these imals. There was never any "red water." A few para-es—exceedingly few—were seen in the cow on June 1, 6th, and 7th; in the heifer on June 2nd, 4th, 6th, d 9th; in the bullock on June 2nd, and doubtfully on ne 4th. Apparently there was only a slight prolifera-n of piroplasmata, and there was no infection to cause en transient fever. There was no blood destruction or eration of hæmoglobin.

On Nov. 8th, 1909, the heifer of the previous experi-nt was tapped, and 200 c.c. of blood injected directly o the jugular vein of an old red cow; 50 c.c. were jected into the jugular vein of a Kerry cow. Neither w gave any temperature reaction, no hæmoglobinuria.

The blood did not give any immunity to the red cow, as a later experiment will reveal, and the blood obtained from the heifer which, if she had a reaction when injected on May 28th, 1909, did not prove to be infective. It will be remembered that animals that have been infected and recover in certain forms of piroplasmosis, still have infective blood—blood capable of producing infection and symptoms in non-immune cattle. The heifer's blood was not infective; nor was the red cow immune to the *P. bigeminum*. Whilst the red cow was under observation after the inoculation the blood was occasionally drawn and examined for parasites. None could be found, and a quantity in bulk was centrifugalised, but the plasma was not tinted with hæmoglobin.

On Feb. 1st, 1910, I obtained blood containing an active virus and injected 20 c.c. into the jugular vein of each of four animals—a black heifer, a red-polled heifer, the roan heifer of the previous experiment of May 28th, and the black Kerry cow. The polled heifer, the roan heifer, and the Kerry cow did not react, and we may dismiss them.

The black heifer's record is as follows:—

Temperature at time of inoculation, 101.0° F. (On 29th of July she was injected with the washings of a clot of blood drawn from a case of piroplasmosis, but she did not react.) Her temperature on the following days reads as follows—Feb. 2, 102.2°; Feb. 3, 102.2°, 102.2°; Feb. 4, 102.2°; Feb. 5, 101.2°, 101.8°; Feb. 6, 101.4°, 101.2°; Feb. 7, 102.4°, 101.0°; Feb. 8, 101.6°, 101.2°; Feb. 9, *105.2°*, 104.0°; Feb. 10, 105.8°, 106.0°; Feb. 11, 105.4°, 105.4°; Feb. 12, 101.0°, 101.0°; Feb, 13, 101.4°, 102.0°. A distinct temperature reaction on Feb. 9th. Piroplasms were found in the blood films even outside the corpuscles in the plasma. On this date there was no

æmoglobinuria and no albumen in the morning, but it as found in the urine in the evening. Next afternoon e urine became dark red; it was albuminous. On eb. 11th it was black in the morning, but in the evening was nearly normal in appearance. Next day it was ite normal in appearance, and has remained so ever nce.

Numerous piroplasms were present in the blood, but ey were very few on Feb. 13, 1910, and disappeared together from the films during the next few days eb. 15th).[a]

The animal had a typical attack of piroplasmosis, a mperature reaction, piroplasms in abundance in the ood, hæmoglobinuria. During the morning of Feb. th, her temperature being 105.8°, I tapped the jugular d withdrew into citrated normal saline about 10 c.c. of ood. The citrated blood in all equalled 20 c.c., and is I injected into the jugular vein of the old red cow entioned previously with the following result. Temrature at time of inoculation:—

12 noon,	Feb.	10	101.0°	-		Evening,	103.2°
Morning	,,	11	100.6°	-	-		101.0°
,,	,,	12	104.4°	-	-	,,	103.0°
						6 p.m.	9 p.m.
,,	,,	13	107.4°	-	-	107.0°	107.0°
,,	,,	14	99.0°	-	-	Died 4 30 p.m.	

On Feb. 12th piroplasmata (numerous) were present in e blood. On the 13th the urine was black, and many roplasms were present. They were also numerous on e 14th, when the animal was evidently moribund. I

[a] Since the above was written the blood examinations have been conued daily, and single piroplasms or pairs of piroplasms have occasionally n seen in the films, March 4, 5, 6, 7, 8, 1910.

injected into the jugular vein, with very little hope I admit, and with little or no expectation of any good result, 140 c.c. of 1 per cent. solution of trypan blue—a remedy advocated for the treatment of piroplasmosis.

The reaction in this case was prompt, very early in fact, and very severe. As will be seen from the preparations, the infection is very remarkable, a very large percentage of corpuscles being infected with typical piroplasmata of the type *P. bigeminum*.

A *post-mortem* examination was made immediately after death and smears obtained from the spleen, kidneys, heart, and red marrow. The spleen was found somewhat enlarged and fairly firm. In some cases it is much increased in size and diffluent. The liver was slightly icteric. The kidneys very dark brown in colour. The other organs were normal. A hypophrenic abscess, of no connection, of course, with the infection, was also found. Sections of the kidney were made and examined, but, with the exception of some cloudy swelling and hyaline casts in the tubules and a certain amount of vascular congestion, there was nothing of importance. Films from the kidneys and heart did not show, as is usually maintained, an excessive number of piroplasms; they were not numerous in either spleen, heart, or bone marrow. The latter appeared normal, perhaps rather more erythroblasts than usual. In the spleen active phagocytosis of red corpuscles by huge macrophages was evident, and though a few of the corpuscles phagocyted contained piroplasms the majority were free from infection.

The experiments are being continued, and I hope when some tangible results as regards immunisation have been attained that I may be allowed to communicate them to the Section.

The microscopic films exhibited included specimens showing the organisms present in the blood of :—

(*a*) Experimental cases.

(*b*) Naturally infected animals of Ireland.

(*c*) Horse's blood (*P. equi*).

(*d*) Ox's blood (*P. mutans*).

(*e*) Ox's blood, East Coast Fever (*P. parvum*).

(*f*) Koch's granules in splenic smears from East Coast Fever cases.

Dr. A. H. Benson expressed the thanks of the Section to Professor Mettam for his paper, which showed the great progress which had been made in pathology during the last few years.

Professor Thompson asked if piroplasmosis was the cause of all cases of red-water in cattle in this country ?

Professor Mettam said it was, as far as he was aware. There was a form in Scotland in which the piroplasm had not been found, but wherever he had examined in piroplasmosis in England or Ireland he had never failed to find it.

Professor O'Sullivan said it would be very interesting if Professor Mettam would tell them something more of the differences between the Irish piroplasmosis and the bigeminum.

Dr. O'Farrell asked if Professor Mettam had made a blood-count. He believed it was usual to find a decrease in the number of white corpuscles and a relative increase in the lymphocytes.

Dr. Parsons said it would appear that red-water fever in Ireland was much less infectious than in South Africa and Texas. Was this, he inquired, due to a difference in the tick or in the parasite ?

Professor Mettam replied that hæmoglobinuria was very common throughout the whole of Ireland. On one farm, where new cattle were introduced from Scotland, 48 out of 59 got red-water, and 8 or 9 of the number died. He was convinced that there was more than one form of piroplasmosis in Ireland. He had seen cases from County Clare, where there was *Piroplasma bigeminum*, but in other parts—from Cork to Antrim—cases were due to a much smaller parasite—one which was bigeminate, but

in the most part circular or spherical. This was the one which he had failed to reproduce, and further experiments were necessary to show whether it was inoculable or not. In "Dum-dum" fever, he did not think the infecting organism was a piroplasm, but one more nearly related to the flagellates. As to leucocytosis, he thought himself there was, if anything, a leucopenia, and apparently nothing like phagocytosis occurred. The white corpuscles certainly did not attack the parasites. The term *Piroplasma bigeminum* was used more generally than specifically. It was the parasite which caused Texas fever, tick fever in Australia, and South African red-water so-called. But quite a host of piroplasmata had been demonstrated in Assam, China, Japan, and in Transcaucasia, where they were of the bacillary nature, probably resembling somewhat the "*P. mutans*." He was convinced that the parasite met with most commonly in Ireland, and called bigeminum, was quite a special one, and he hoped to elucidate the matter within the next few months. The disease was not very fatal in Ireland, probably in not more than 5 per cent. of the cases. A carcase would not be passed in the condition, as the spleen was very large, and the dresser would take it to be anthrax, and get rid of the carcase.

Dr. Wigham asked if it would be difficult to communicate the Irish disease by the action of ticks. He would like to know if experiments had been done in filtered blood to find if the virus was able to pass through it.

Professor Mettam said the virus was not passed through the filter. For some time past they had been breeding ticks, but when ready to be put on the cattle they appeared to commit suicide! In cases of canine piroplasmosis, many dogs had been saved by tripan blue. Animals injected with it became blue, but the colour passed off in a short time.

Dr. O'Farrell asked if there was any difference in the virulence in the bites of the embryonic and adult tick.

Professor Mettam said the history of the transmission of the disease varied with the tick. Some inoculated the disease at one stage, some at another, but there was no difference as regards the virulence.

Professor Thompson asked if the disease was associated with any particular kind of land.

Professor Mettam replied that red-water was always associated with pastures where there was cover for ticks, such as hedges and old grass. The land could be cleared by firing.

Dr. Stokes asked if the ordinary treatment for red-murrain in the country was useless.

Professor Mettam replied that he believed the animals would in some cases get better equally rapidly if there was no treatment.

PATHOLOGICAL REPORT OF THE ROTUNDA HOSPITAL FOR THE YEAR ENDING 31st OCTOBER, 1909.

By ROBERT J. ROWLETTE, M.D., Univ. Dubl., Pathologist.

[Read in the Section of Pathology, April 8, 1910.]

I am glad to report a considerable increase in the work undertaken by the Laboratory of the Rotunda Hospital during the year 1908-9.

Autopsies were performed in the case of eleven adult patients, seven from the gynæcological side and four from the maternity. Brief notes are appended:—

Case I. (Gynæcological).—B. M., aged thirty-five, died 18th November, 1908. Autopsy same day. Patient had myomectomy performed three days previously, and lost much blood. Several copious saline injections were given. Only lesion found—œdema of lungs and pulmonary embolus. No peritonitis.

Case II. (Maternity).—K. K., aged thirty-four, died 8th December, 1908. Autopsy four hours after death. Had been confined of dead child with hydramnios 18th November, 1908.

Lungs—bronchitis.

Heart healthy.

Abdomen—general peritonitis. Purulent exudate most marked in pelvis.

Uterus somewhat enlarged; wall soft; interior clean.

Other organs normal.

Dipplococcus a. was recovered in pure culture from the peritoneal exudate and the endometrium.

Case III. (Gynæcological).—M. A., aged forty-eight, died 5th February, 1909. Autopsy next day. A large tumour of right kidney (hypernephroma) had been removed thirty-one hours before death. Patient only passed 6½ oz. urine in the intervening period.

Lungs.—Secondary tumours in both lungs.

Left kidney small, with chronic interstitial nephritis.

Other organs healthy.

In this case I believe the death was caused by the sudden strain thrown on the diseased left kidney. In the removal of the tumour a fair amount of comparatively healthy kidney substance had to be taken away.

Case IV. (Gynæcological).—M. K., aged thirty-six, died 12th March, 1909. Had been delivered of twins two months previously, and not felt well since. Admitted to hospital moribund.

Thorax.—Empyæma of left lung.

Abdomen full of pus.

Case V. (Gynæcological).—C.B., aged forty-six, died 23rd March, 1909. Autopsy same day. Myomectomy 16th March, 1909.

Thorax healthy.

Abdomen.—General purulent exudate, with large quantity of free pus. Abscess in left tube and broad ligament.

Case VI. (Gynæcological).—M. H., aged forty, died 8th May, 1909. Autopsy twenty-four hours later. A malignant, infected cyst had been removed 7th May, 1909.

Thoracic organs normal.

Abdomen.—General peritonitis with considerable purulent exudate. Very marked inflammation of lower part of peritoneum.

Organs normal.

Streptococcus recovered in pure culture and in smears from exudate.

Case VII. (Gynæcological).—E. C., aged fifty-two, died 28th July, 1909. Died after removal of cyst while waiting for pylorectomy.

Peritonitis.—Perforation of cancerous ulcer of pylorus.

Case VIII. (Maternity).—E. M., aged twenty-one, died 5th August, 1909. Autopsy same day. Patient had been delivered of putrefying fœtus after rupture of uterus.

Thoracic organs healthy.

Abdomen filled with foul-smelling fluid. Uterus large, œdematous, very friable; large rent in posterior wall, through which a piece of the putrefying placenta had escaped.

Other organs normal.

No pathogenic germ was recovered, but putrefactive organisms were plentiful.

Case IX. (Maternity).—M. M., aged nineteen, died 28th August, 1909. Autopsy eighteen hours later. Died after rupture of hydramnios. No trace of disease was discovered except a few drops of pus in the pelves of both kidneys.

Case X. (Gynæcological).—M. C., aged fifty-three, died 19th September, 1909. Autopsy next day. Died two days after admission to hospital.

Lungs.—Several masses of cancer in both lungs.

Abdomen.—Masses of cancer.

Uterus completely disorganised by cancer.

Curettings and other fragments were examined on eighty-one occasions for diagnostic purposes:—

Table I.—Examination of Curettings and other Fragments for Purposes of Diagnosis.

Endometritis . .	49	Erosion of cervix .	1
Adeno-carcinoma of uterus	1	Epithelioma of vulva .	1
Mucous polypus .	4	Adeno-carcinoma of vulva	1
Hydatidiform mole .	3	Tuberculosis débris .	1
Epithelioma of cervix .	1	Blood-clot, granulation tissue, normal, &c. .	17
Papilloma of cervix .	2		

Proceeding on the same principle of classification as

heretofore, the 49 cases of endometritis are classified as follows:—

Table II.—Endometritis.

Glandular (including cystic) . .	30	Septic . . .	13
Interstitial . .	2	Post-abortive . .	3
		Tubercular . .	1

I am aware that the classification of endometritis is in an unsatisfactory condition, and the terms used above are used rather as suggesting the most striking characteristic of the sections than as expressing fundamental distinctions. The cases described as "septic" are those in which no other changes were discovered than those we commonly associate with acute inflammation, congestion, œdema, extravasation of blood, leucocytic infiltration, and necrosis. The condition in the cases described as "interstitial," on the other hand, is such as might be produced by a chronic inflammatory process, fibrosis of the connective tissue of the endometrium with atrophy of the glands. More than half of the specimens dealt with, however—30 out of 49—are described as "glandular," as all of them presented a marked increase of glandular elements. In many of them there were also inflammatory lesions present, but in the present state of our knowledge we can hardly regard glandular hypertrophy as explicable purely on the hypothesis of inflammation. Its cause is still to seek. Moreover, some of the conditions described as "glandular endometritis" are indistinguishable from the hypertrophy that occurs in early pregnancy. In two of the three cases of post-abortive endometritis the changes were such as are often seen in glandular endometritis. In two of the four cases of cystic endometritis there was a clinical

history of heavy bleeding. On three occasions "hydatidiform mole" was encountered; the three curettings were, however, from one patient, and the operation was repeated at intervals of a month. On each occasion typical structures were found, but after the third curettage bleeding entirely ceased, and now after six months the patient remains quite well. Of the diagnosis of the specimen reported as "tubercular" I must admit a doubt. The clinical history of the patient as regards wasting suggested the possibility of malignant disease. In one of the first sections made I discovered a patch of apparently caseous material, surrounded by a zone of small round cells. No giant cells were seen. Sections were made of all the fragments which had been removed, but no other lesion suggesting tuberculosis was found. The patient gave a positive ophthalmo-tuberculin reaction. She has remained quite well since the curetting. The occurrence of tuberculous endometritis is very rare. In my five years' experience at the Rotunda I have met no specimen except this in which even a suspicion of tuberculous endometritis arose.

Tumours and operation specimens were examined in 101 cases—a considerable increase over previous years:—

TABLE III.—EXAMINATION OF TUMOURS AND OPERATION SPECIMENS.

Epithelioma of vulva .	1	Tubercular salpingitis .	6
Adeno-carcinoma of vulva	1	Tubo-ovarian abscess .	1
Cyst of vagina . .	1	Hydro-salpinx . .	2
Epithelioma of cervix .	8	Cyst of ovary . .	28
Adeno-carcinoma of uterus	1	Fibroma of ovary .	1
Fibro-myoma of uterus	36	Prolapsed rectum .	1
Tubal pregnancy .	6	Carcinoma of pylorus .	1
Pyo-salpinx and salpingitis	6	Retro-peritoneal tumour	1

Adeno-carcinoma of the vulva is a rare disease. We have only seen it once before in five years at the Rotunda, whereas we have met with epithelioma of the vulva on six occasions. The macroscopic appearances of the two diseases are similar, and the conditions are only to be distinguished by the microscope.

There is an unusual preponderance of epithelioma over adeno-carcinoma of the uterus—7 to 1. In the last five years we have had 31 cases of epithelioma to 14 of adeno-carcinoma. Of the 36 fibroids encountered, 13 showed more or less degeneration. Two showed fatty changes, one mucoid, four grey necrosis, one red degeneration, two were markedly œdematous, and four contained cystic spaces. These spaces are not true cysts, as they never have a definite living membrane; they appear to be merely dilatations of the lymph-spaces of the tissue by excess of fluid, filled in most instances by clear fluid, less frequently by mucoid material. In nearly all the previous cases of red degeneration which came under our notice the fibroids were co-existent with pregnancy. In the present case there was not pregnancy, but there was a great mass of inflammation of the tubes and ovaries.

Of the 28 cysts of the ovary, 14 were smooth-walled multilocular cysts, and 4 were papillary. One dermoid was met, one was part dermoid and part multilocular, and one was a malignant papillary cyst. The remainder were small cysts, such as lutein cysts, and cysts of Morgagni removed during operation undertaken for other purposes. The malignant papilloma had caused secondary growths in the uterus. The history of this case is interesting. In December, 1907, a cyst had been removed from the patient. It was for the most part smooth-walled, but there were a few papillary tufts. Microscopic examination was made of

these tufts, but nothing was found to suggest malignancy. The patient returned in June, 1909, with considerable ascites. A cystic growth was removed, together with an enlarged uterus. The latter was found to contain a soft, fungating mass, in which, however, no definite cystic or papillary structure was discoverable, though to the microscope there was no doubt of its malignancy. The cystic growth showed glandular spaces with proliferating epithelium, also malignant. A systematic examination of the original specimen was then undertaken, and in a few slides evidences of malignant epithelial infiltration were discovered. In my last year's Report I mentioned two cases in which a cyst of the ovary, regarded as innocent, showed power of recurrence. This year, in addition to the case first related, another case of the same sort occurred. In January, 1909, a large cyst with nearly smooth walls, but for a few papillary tufts, was removed from a healthy woman four months pregnant. She returned to the maternity wards in June at full term, and delivered herself of a healthy child. In December she reported herself as perfectly well; nevertheless, early in January of the present year she came into hospital with a tumour in her liver and the right side of her thorax full of fluid. The pleura was tapped, and large quantities of bloody fluid removed, but her chest speedily filled again. After three tappings an exploratory incision into the abdomen revealed masses of cancer in her abdominal wall and liver. The patient died suddenly on getting out of bed. *Post-mortem* we found small plaques of cancer in the pelvis and large masses in the right hypochondrial region. The liver and diaphragm were bound together in a mass of cancer, in which was buried a cyst containing over a pint of bloody fluid. The right pleura was everywhere infiltrated with cancer, the

ung being shrunken and encased in the growth. The pericardium was slightly affected. Nowhere else in the body, nor even in the abdomen, except in the pelvis and right hypochondrium, were any tumours found. In view of the frequency in our recent experience with which apparently innocent cysts have proved themselves malignant, I believe that all cysts of the ovary showing any papillomatous growths or any solid patches whatever should be regarded as, at any rate potentially, malignant. At the same time, without making an enormous number of sections, this malignancy is not likely to be discovered by the microscope.

The case of carcinoma of the pylorus is interesting as occurring in a patient from whom a cancerous gall-bladder had been removed twelve months previously.

The uterine lochia was examined bacteriologically in 35 morbid cases:—

Table IV.—Bacteriological Examination of the Uterine Lochia in Thirty-five Morbid Cases, showing the Organisms Observed or Isolated:—

Diplococcus a	16	Bacillus coli	4
„ pneumoniæ	10	Unrecognised bacilli (probably saprophytes)	7
Staphylococci	5	Negative	15
Streptococcus	2		
Gonococcus	1		

These figures are similar to what we have found in previous years. The *Dipplococcus a.*, as usual, was associated with a mild degree of morbidity. But there was one notable exception in a case already mentioned in the *post-mortem* notes. In it *Diplococcus a.* had been isolated from the uterine lochia in pure culture. After some weeks' illness death occurred, and, as has been

stated, the organism was also recovered in pure culture from the peritoneum. This is the first fatal case of infection with this organism which, as far as I can discover, has been recorded. The two cases of streptococcal infection were mild—an unusual occurrence. In one of them there was a very mixed infection, and I incline to think the material submitted to me was in part vaginal. After a few days' douching the patient became quite well.

Dr. Stokes asked for further information with regard to the pathology of the hydatidiform mole.

Dr. Boxwell said he was particularly struck with the comparative rarity of carcinoma of the uterus, considering the exceeding frequency of chronic endometritis. All the factors favouring carcinoma seemed to be present—the chronic inflammation and the epithelial proliferation—and yet it appeared rarely to go on to carcinoma. He wished to know whether adenocarcinoma of the body of the uterus was often directly traceable to antecedent endometritis, and whether these cases occurred at the menopause, or at a later period. With reference to the specimen of the ruptured tubal pregnancy, the frequency of rupture was said to be due to the peculiar tendency to bore possessed by the fertilised germinal cell. Was it a mere coincidence that the germinal cells and the cells of cancer had then two characteristics in common—that of boring into surrounding tissues and a special form of mitotic nuclear division—characteristics possessed by no other cells in the body to the same degree? He felt some apprehension on hearing Dr. Rowlette's remarks with reference to the malignant tendencies of the common multilocular ovarian cyst. These tumours, under the microscope, looked as if they might become malignant very readily, and yet he had nearly always failed to demonstrate any downright malignant change.

Dr. O'Farrell asked what particular solid-looking piece, in a case of cystic ovary, would Dr. Rowlette choose for microscopic examination in testing for malignancy. In the case of hypernephroma of the lung, he would like to know if there

were any secondary deposits in the bones. He would also like to know if the carcinoma of the liver was secondary to the original cancer, or secondary to the secondary deposit in the lung, passing through the diaphragm.

Mr. Mooney inquired as to the present condition of affairs with regard to the diagnosis of gonococcus. They at the Eye Hospital had no difficulty in that respect, as it was generally coupled with clinical signs.

Dr. Rowlette, in reply, said he believed that hydatidiform mole was a cystic proliferation of the chorionic villi; the villi proliferated unduly, and at the same time became cystic, and the rest of the ovum disappeared, so that one found, as a rule, nothing left but a mass of cysts. Occasionally one found a fœtus of a month's or six weeks' development, together with a mass of the mole formations. The condition was undoubtedly one of a mild degree of malignancy. In many cases the uterus did not completely empty itself, and if any remained it was likely to grow and make secondary deposits. Chorion epithelioma was a more malignant form of growth, consisting of structures also present in the mole, but without the cystic formation, and in many cases where mole had been observed the patient was found afterwards to suffer from chorion epithelioma. The rarity of carcinoma of the uterus as compared with chronic endometritis was even greater than appeared from his figures, as not all the curettings were sent to him for examination, when there was no possible suspicion of malignancy, whereas his figures gave the entire number of cases of carcinoma in which operation was performed. Endometritis occurred, he thought, chiefly before the menopause, while adeno-carcinoma occurred at or about the menopause or later. One would expect that the epithelial proliferation would go beyond bound and become malignant oftener than it did. In choosing a piece of cyst to examine, he generally chose the most solid part of what was apparently anepithelial or a papillomatous structure. He did not think it was possible, without a more minute examination than was practicable in hospital practice, to rule out the possibility of malignancy. The making of a couple of hundred sections of a cyst was not possible, and one had to choose the most suspicious-looking part of the tissue. He thought the malignancy

of the cyst was held in check to a great extent by the densit of the fibrous wall, so that as long as the cyst wall was not broke down it was less likely to spread than after an operation in whic any part of the cyst was left behind unprotected by the wal which originally closed it in. He did not think the malignan growth of the liver and diaphragm was secondary to cance of the lung, as the cancer was apparently entirely of pleura spread in the thorax—not having reached the lung by the blood vessels. The cyst was very large, and could not be remove without allowing the contents to escape, and it was possibl that some of the papillary growths escaped in the first operatio and were carried to the right hypochondrium. In the case o hypernephroma a careful examination of the bones was no made. With regard to the diagnosis of gonococcus, in the report of some years ago on puerperal infection, the gonococcus wa credited with a great amount of the morbidity. He was inclined to think that that was due to the mistake of regarding any diplococcus of its shape and staining reactions as being gono-coccus, without regard to cultural characteristics or the position as regards the leucocytes. The diagnosis of gonococcus in the two cases last year, and the one case this year, was made by a process of exclusion. The diplococcus *A* was rather like the gonococcus in appearance; it did not grow very readily, but could nearly always be got to grow, to some extent, on ordinary agar; the gonococcus would not grow in that way, but only on such media as ascitic fluid or blood agar. Diplococcus *A* was more rounded than the gonococcus, and the leucocytes did not seem to have any special power of dealing with it.

HYPERNEPHROMA—PYONEPHROTIC KIDNEY.

By T. T. O'FARRELL, L.R.C.P. & S.I.
Pathologist to St. Vincent's Hospital, Dublin.

[Read in the Section of Pathology, April 8, 1910.]

Hypernephroma.

This kidney was removed by Surgeon M'Ardle, and it is by his kindness that I am enabled to submit the specimen.

The patient, M. M., aged fifty-four; occupation, general house work; admitted into hospital 2nd November, 1909, discharged 9th December, 1909. Patient was married, had had eleven children; eight years since last confinement.

History.—Since December, 1908, patient noticed that she was getting thinner, but was in fairly good health till the end of May, when she began to vomit immediately after meals. The vomited matter consisted mainly of food stuffs, but on one occasion she had noticed that it was black in colour.

Early in June she had two attacks of epistaxis. She consulted a doctor, who drew her attention for the first time to a swelling in her left side, and recommended her to go into hospital.

She was admitted to the Gynæcological Department, but Dr. Alfred Smith, having examined her, pronounced the swelling not to be of pelvic origin, so she was transferred to the Surgical Ward under the care of Surgeon M'Ardle.

Present condition.—The patient complains of pain in the left side, increased by exertion or by lying on the left side. Her bowels act well, but a full bladder gives rise to pain in the left side; she also suffers from thirst and inability to retain solid food. The urine contains some albumen and pus.

On Examination.—A tumour was felt in the left side of the abdomen, extending from the upper edge of the pelvis to the costal arch and extending to the middle line in front. The tumour moves with respiration, and is slightly movable in an upward and side to side direction. There is a projecting node from the lower end of the tumour, which feels like the lower pole of a displaced kidney, but it cannot be defined above, as it is

continuous with the general tumour. The tumour is dull on percussion, but it has the resonant colon in front to the outer side.

A diagnosis of kidney tumour was made, and it was determined to operate. Nephrectomy of left kidney performed by M'Ardle's method on 8th November. Recovery uneventful.

Pathological Report.—The specimen is about four times the size of a normal kidney. The upper two-thirds is taken up by a large tumour, the remainder being kidney, which appears as a projection from the tumour. The weight of the whole specimen is 1,075 grammes, and it measures 18.5 centimetres in length by 13 centimetres in its broadest diameter. It is to be noted that one-third of the kidney, about 6 centimetres, is from the outside apparently free from tumour.

There are many adhesions about the tumour, one particularly large at the convex border. On cutting open the specimen from convex border to pelvis it is seen to consist mainly of tumour, with very little normal kidney tissue, only about three centimetres being free from new growth, very much less than would appear from the outside.

The first thing that attracts the eye is a large, dark, circular patch, measuring seven centimetres in diameter, which is apparently encapsulated, and in position is quite close to the edge of the specimen.

The remainder of the growth is divided irregularly into lobules by thin connective tissue bands. It is limited by a kind of capsule on the outside, but on the inside the tumour encroaches upon the pelvis, into which portions of the tumour are seen to project. There are some small islands of tumour in the otherwise healthy-looking kidney substance. The whole tumour presents a variety of colours, from a dark red in the circular patch to a comparatively light grey colour at the pelvis. Throughout the specimen there are numerous yellowish caseous-looking spots.

The specimen presents the typical naked eye appearance of a hypernephroma.

The microscopical sections are particularly interesting, as they exemplify practically all the characteristics which are usually found both in the early and late stages in the growth of these tumours.

Section 1, *taken from the circular, apparently encapsulated, portion.*—The principal cells of the tumour are seen to consist

Mr. T. T. O'Farrell—"Hypernephroma—Pyonephrotic Kidney."

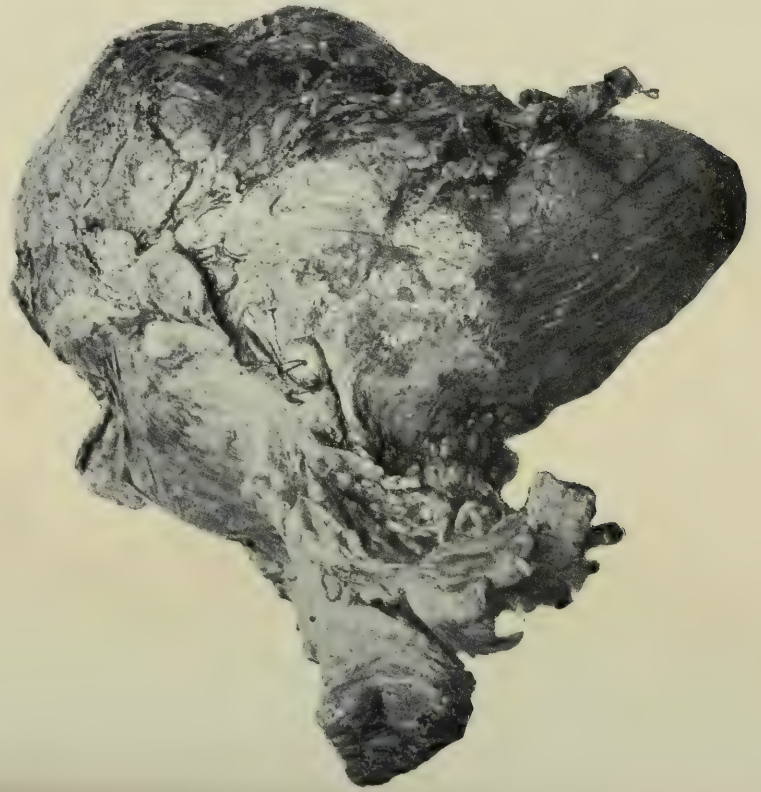

Fig. 1

of large globular or polygonal cells, with a distinct outline, and containing a well-defined spherical nucleus; these are termed "swollen cells." The cells are somewhat irregularly arranged in rows and columns, and are closely associated with the capillary vessels; it is this arrangement that recalls the appearance of the suprarenal cortex. The capillaries and fine strands of connective tissue between the cell columns run in a more or less parallel direction with the capsule, which is in places infiltrated by tumour cells. Towards the innermost portion of the section the tumour is broken up into large blood spaces, like an angioma, but in all probability this is due to extravasated blood. A very noticeable point is that there is a more or less marked infiltration by small round cells, in many places found in clumps.

Section 2, *taken from the upper pole of the specimen*.—This section has somewhat the nature of section 1, but the connective tissue and small capillary vessels are distributed more irregularly among the cells, and have a tendency to run more at right angles to the capsule. Besides the "swollen cells" there are distinct "opaque cells" (not seen in Section 1). There are no large blood spaces in this section. Towards the innermost portion of the section there is a quantity of degenerated tissue. The fibrous capsule is free from tumour cells, but contains one or two patches of small round cells which are scattered throughout the section as in No. 1.

There are also several large cells somewhat similar to hyaline leucocytes in the section—*i.e.*, a large cell with more or less clear protoplasm, having a bean-shaped nucleus which takes up about one-third of the cell area.

Note.—These cells form a large percentage of tumour cells found in the alveolar arrangement of Section 3.

Section 3., *taken from the island of tumour in the kidney substance*.—Shows more or less normal kidney substance slightly congested. One portion is taken up by tumour, which shows a distinctly alveolar arrangement. In places the tumour cells push the fibrous tissue cells in front of them, forming a kind of capsule, but in several places they have infiltrated the kidney substance, and so have apparently assumed a malignant character.

Section 4., *taken from a part adjacent to Section* 3.—Shows kidney substance encroached upon by tumour.

A noticeable feature of this section is the appearance of giant cells scattered over the field, but more numerous at the advancing edge of the tumour.

Twenty-four years ago Grawitz made a very careful study of these hitherto unclassified tumours, and came to the conclusion that they had origin from adrenal rests in the kidney substance. This view has been confirmed by Lubarsch, Gatti, McWeeney and Kelly.

The main points in favour of the adrenal rest theory are :—

(*a*) The tumours correspond in position to the subcapsular islets of suprarenal tissue sometimes found in the kidney.

(*b*) There is a striking difference between the tumour cells and those of the renal epithelium.

(*c*) The presence of fat in the tumour cells and suprarenal cortical cells as against the absence of fat in the healthy kidney epithelium.

Now, Stoerk, in 1908, brought forward some fairly strong evidence against the suprarenal rest theory. This has been summarised in Dr. Trotter's very instructive lecture of the 10th February, 1909, on hypernephroma, and it is from that that the following points are derived :—

Points against the Suprarenal Rest Theory.

(i.) Why do hypernephromata always originate in the kidney and never in the suprarenal ?

(ii.) Why are tumours of the suprarenal (both benign and malignant) always different in structure from hypernephroma ?

(iii.) Suprarenal rests are found in the liver, but hypernephroma never occurs there.

(iv.) Suprarenal rests are commonest in the upper pole of the kidney, whereas hypernephroma chiefly affects the central part.

(v.) The sub-capsular theory is negatived by the fact that while suprarenal rests may occur actually in the substance of the capsule of the kidney, hypernephromata never do.

(vi.) Stoerk holds that "swollen cells," which are most characteristic of hypernephroma, are quite unlike anything in the suprarenal body.

(vii.) That, although it is true that hypernephroma cells show doubly refractile substance as do the suprarenal cells, many other epithelial tumours show the same phenomenon.

Fig. 2.

(viii.) Stoerk holds that hypernephroma is essentially a tumour of tubular structure, tending to produce spaces and intracystic formations, while nothing of the kind occurs in the suprarenal.

(ix.) He produces evidence to show that in small "adenomatous" masses found in the kidney, undergoing degenerative changes, structures are to be found resembling very closely many characteristic parts of hypernephroma.

(x.) He concludes that the tumour must be classified as an adenoma, cystoma, papillare or carcinoma, according to the form in which it occurs.

Dr. Trotter, though not altogether falling in with Stoerk's views, thinks that his article has left the classification of this tumour in a somewhat doubtful state.

Pyonephrotic Kidney.

Dr. O'Farrell said his only excuse for bringing the specimen was to demonstrate the usefulness of clinical pathological methods—viz., blood count and urine examination.

As this was a particularly obscure case the investigation of the blood and urine greatly assisted in the correct diagnosis, and for that reason will be of interest to the members of the Pathological Section.

Kidney, No. 11, *St. V.*

Weight	-	1,015 grammes.
Length	-	16 centimetres.
Breadth	-	12 ,,

Kidney shows much lobulation, about twenty prominences, varying in size from hazel nut to small orange.

Kidney soft and fluctuating, capsule dense, 2 to 3 mm. thick, some dilated vessels on surface. Specimen full of pus.

Kidney, Case No. 11, *St. V.*

M. S., aged twenty-three; admitted into hospital 8th March, 1910.

History.—Patient never had been very strong, and has recently been wasting. About twelve months ago she complained of pain at the lower margin of the ribs, on the right side, which was more marked after standing, but was relieved by sitting down. Constipation became more marked than it ever had been

previously, and menstruation ceased. It was for this latter condition that the patient consulted a doctor, who prescribed for her. The pain was relieved, but the menstruation did not return. Subsequently a swelling was felt in the right lower abdomen, and in view of the symptoms some disease of the pelvic viscera was suspected. She was recommended to go into hospital.

Present condition.—A tumour can be palpated in the right side of the abdomen, which is internal in position to the colon. The colon is not connected with the pelvis.

A blood examination was made, which revealed a large increase in the number of polymorphonuclears, indicating the presence of pus somewhere. The urine was examined, and was found to be acid in reaction, and to contain albumen and much pus. Pus in acid urine may come from (*a*) the bladder in cystitis, due to infection by gonococcus, *B. coli*, and *B. tuberculosis*; or (*b*) from the kidney. Gonococcus and *B. coli* were eliminated, and as *B. tuberculosis* could not be found, it was in all probability not tubercular cystitis, as in that condition it is generally easy to detect the bacillus. This left the diagnosis of pus in the kidney. By von Pirquet's reaction the nature of the disease—viz., tuberculosis of the kidney or ordinary pyonephrosis—could have been made, but it was not tried in this case. The large amount of pus in the urine precluded the use of the cystoscope. The segregator was used with no definite result. As much pus was coming in the urine, and as the patient was not improving, after consultation with, and assisted by, Mr. M'Ardle, Mr. Fagan removed the kidney.

The patient, since the operation, has been doing well, and is putting on weight. The subsequent history Dr. OFarrell hoped to bring before the Academy on a future occasion.

Dr. Cahill asked if the urine had been examined for sugar.

Dr. Stokes asked if the cause of pyonephrosis had been found.

Dr. O'Farrell, in reply, said they had tried to get some organism in the pyonephrosis, but the cultures had failed to grow. No sugar was found in the hypernephroma case.

ABSTRACTS.

SECTION OF PATHOLOGY.

Friday, November 12, 1909.

The President—Dr. A. H. Benson, in the Chair.

President's Address.

The President delivered an address "Concerning Trachoma," see page 333, *ante*.

Hypertrophic Cirrhosis with Alcoholic Paralysis.

Dr. Drury read notes of a case of this disease. A. B. was a large stout woman, aged forty-two. She had had twenty pregnancies, with thirteen living children and seven mishaps; these latter having been irregularly distributed between the five births. There was no history or evidence of syphilis. She was admitted to hospital in a state of alcoholism, and she had an alcoholic history. She complained of weakness and "pins and needles" in her feet and hands. There was an abrasion on both knees from her legs having "given under her" on getting out of bed. Her mind was quite clear. The lungs were healthy. The heart was strong and regular, but there was a mitral systolic murmur. There was not, however, any evidence of failing compensation. The movements of the limbs were good, and the hands could be extended to the full, but the muscular action was weak. The muscles of the arms, especially the extensors, were painful to pressure. Those of the legs were in a similar condition, and attempts at voluntary flexion caused pain in the calf. The reflexes were absent; while sensation, though perverted, was still present. It was evident that the patient was in an early stage of peripheral neuritis. This gradually developed from day to day until there resulted complete "drop wrist" and "drop foot." She could draw her legs up in bed, but could

not extend them again. The abdomen was very large and pendulous; at the umbilicus was a tumour about the size of a hen's egg, which was the empty thickened sac of an old-standing umbilical hernia. The liver and spleen were both greatly enlarged; there was no ascites. Jaundice was present, the conjunctivæ being of a bright yellow colour, but the coloration of the skin was such that it might easily escape notice. The urine was high-coloured and clear, without albumen or sugar; it was faintly alkaline to litmus, and a drop of acetic acid brought down a dense cloud of urates; it gave the iodine reaction for bile. The fæces were lighter than normal in colour, but contained bile colouring matter, and were not China-clay like. The liver was very large, reaching in the para-sternal line more than two hand-breadths below the lower border of the ribs. It was readily palpable; was smooth and firm, with a sharp hard margin, but of normal shape, and painless. Simple hypertrophy was excluded by the extreme firmness of the organ and the remarkable sharpness of the edge; it was, moreover, larger than one was likely to find in simple fatty liver. Amyloid disease was excluded by the absence of any cause for such, the history of the pregnancies being alone sufficient to preclude syphilis. The painless condition, great enlargement, and absence of cardiac symptoms excluded congestion. There remained hypertrophic or biliary cirrhosis, and all the symptoms and signs supported this diagnosis. As time went on the jaundice did not materially increase. She had a continuous but quite irregular febrile temperature for some five days, and then (October 10th) developed a profuse diarrhœa without assignable cause. The only medicine she was taking was quinine in small tonic doses, and this was immediately stopped. The diarrhœa continued uncontrolled, in spite of opium, for seven days, and then ceased, apparently under the influence of lead and opium pill. The attack of diarrhœa caused a great change in her condition. She became rapidly and alarmingly weak, and fell into a heavy lethargic or semi-comatose state, but complained of sleeplessness and nightmare. The sleeplessness did not yield to hypnotics. The heart sounds became so weak that the murmur, which was at first loud, could not be heard. The temperature fell to sub-normal, a collapse temperature rather than a normal or afebrile condition. With the increasing neuritis she became quite helpless, and so sank

gradually lower, till she died rather suddenly on the 24th of October. The great enlargement of the liver having the characters already described, the enlarged spleen, the jaundice without ascites, the irregular fever, the profuse diarrhœa, the bile coloured fæces, and the great asthenia passing into semi-coma, and death, together produce the clinical picture described by Hanot, which has been named hypertrophic, or biliary, or insular cirrhosis of the liver.

Prof. O'Sullivan gave the following account of the *post-mortem* findings:—

"The liver weighed 7 lbs. 11 ozs. The increase in size was fairly uniform in both lobes, but was much greater antero-posteriorly than from side to side. Its capsule was somewhat thickened; it was finely granular on the surface, and of a light greenish-yellow colour.

"On section, the connective tissue was seen to divide the liver up into small areas about as big as, or smaller than, a lobule. Under the microscope there was a great quantity of connective tissue arranged on the whole with a tendency to surround lobules. It also sent delicate strands everywhere in between the liver cells. There was extensive fatty infiltration, and the liver was stained green in places with bile pigment. The connective tissue outside the lobules contained great quantities of newly formed bile ducts.

"A section of the external cutaneous nerve showed extensive degeneration, all the larger fibres being degenerated; the myelin and axis cylinders were broken up, and the nuclei of the sheath were increased."

Perforated Gastric Ulcer.

Dr. J. B. Coleman, C.M.G., exhibited certain viscera of a woman, aged thirty-four years, who died with symptoms of gangrene of the lungs, consecutive to perforated gastric ulcer. The autopsy revealed a small ulcer, which had perforated, on the posterior wall of the stomach, two inches from the fundus. Its sinuous track from the perforated ulcer led, by a perforation in the diaphragm, into an abscess cavity, one and a half inches in diameter, between the diaphragm and the base of the left lung. The lower lobe of the left lung contained several abscesses and gangrenous areas.

Stercoral Ulcers in Small Intestine.

Dr. Robert Rowlette showed a specimen of the above. The organ had been obtained, *post-mortem*, from a woman who had suffered from diarrhœa for seven years. For the last two years she had not passed a solid motion. The lower three feet of the ileum were crowded with small ulcers, which had destroyed the greater part of the mucous membrane of the region. The ulcers were clean cut, reaching to the muscular layer, and some of them contained sloughs. In the cæcum was similar ulceration of less extent. In the ascending colon, close to the ileo-cæcal valve, was a stricture, whose lumen did not admit a lead pencil. The stricture was found to be due to scirrhous carcinoma. The condition of ulceration Dr. Rowlette believed to be identical with that occurring in the large intestine as the result of stasis of the contents. Of the four theories put forward to explain the ulceration—(1) irritation by scybala, (2) obstruction of the vascular supply by stretching, (3) bacterial invasion, (4) toxic solution, he inclined to accept the last.

Friday, December 10, 1909.

The President in the Chair.

A Case of Orbital Tumour.

Dr. A. H. Benson exhibited two tumours removed from the orbit and parotid region of a boy aged nine. When he saw the boy the eye was pushed forward and downward. A tumour was felt so firmly adherent to the brow that it was impossible to get any motion between it and the bone. The edge of the bone could not be made out as distinct from the tumour, and several who saw it were of opinion that it was a bony tumour. He got the case X-rayed, and it was declared not to be a bony tumour. He showed the case at the Ophthalmological Club, and his colleagues were about equally divided as to whether it was a cyst, a sarcoma, or a bony tumour. On cutting down on it he found it to be a very dense and almost bloodless tumour, extremely adherent to the bone, and lapping round the edge of the orbit coming

from inside. It was excised with considerable difficulty One did not know if it would recur, but from the comparative completeness with which it was shelled out there was, he thought, every hope of its not recurring. The second tumour was from the parotid region of the same side; it was definitely localised, but was adherent to the gland substance.

Mr. H. C. Mooney said the slides under the microscope showed the tumours to be sarcomata of a large round cell type. Sections of the gland presented similar appearances to the section of the tumour, except that there were some gland elements intermixed and more of the fibrous tissue.

Thyroid Tumour growing in the Tongue.

Mr. Gunn gave a brief description of the development of the thyroid gland, and in particular of the middle lobe or isthmus, showing how, according to the theory propounded by His, it was not a difficult matter to understand the growth of tumours composed of thyroid tissue at or below the foramen cæcum. In Mr. Gunn's case such a tumour had occurred in a girl of eighteen. On removal it proved to be carcinomatous thyroid tissue. As far as he could find, no similar case had been recorded. A further point of interest in the case was the absence of the thyroid gland from its normal situation. This was demonstrated during the course of a tracheotomy performed as a preliminary to the removal of the tumour.

Dr. W. G. Harvey and Professor Scott agreed that it was undoubtedly carcinomatous tissue.

Dr. Boxwell expressed the thanks of the Section to Mr. Gunn for the exquisitely made model shown, which demonstrated the subject to perfection.

Mr. Gunn, in reply to a question by Dr. Stokes, said he had had no personal experience of secondary deposits of non-malignant character associated with carcinoma of the thyroid. One patient, however, had died with a very small growth in the tongue and very malignant deposits in the lungs.

Three Rare Pathological Conditions in the Bone.

Dr. W. G. Harvey, in introducing this subject, said:—The skiagrams of three cases which were recently sent to me for

radiographic examination present points of such especial interest that I thought it worth while to show them here.

Case I.—A case of *Multiple Osteomata.*—A boy, aged six, a patient of Mr. T. E. Gordon, and the subject of multiple osteomata. The condition is itself not uncommon, but the multiplicity of the tumours and the degree of deformity in this case are unusual. Tumours are situated on the upper dorsal vertebræ, both scapulæ (numerous), the upper ends of both humeri, the lower ends of the radius and ulna; on the metacarpals and phalanges on both sides; also on the lower ends of both femora and at both extremities of the tibiæ and fibulæ. Osteomata, or bony tumours, are described as of three kinds—(1) ossifying chondromata; (2) ossifications of tendons and muscle insertions, and (3) calcifications of inflammatory exudates. *Myositis ossificans* must be classed separately. In this case the position of the tumours, occurring in the long bones almost invariably near the epiphyseal end of the diaphysis, leaves little doubt but that their origin was from cartilage cells possessed of aberrant growth situated in the diaphysis close to the line of growth of the bone. Especially are the radiographic appearances of the upper end of the tibia and fibula suggestive of enchondromatous origin. The spur on the lower end of the right femur is, however, evidently developed in the adductor sheath. I cannot regard this as a fortuitous combination. I have often found such in other radiograms, and I suggest that both forms of osteomata originate in the same way—that there is no essential difference in the pathogeny of bony spurs occurring in muscle insertions and true ossifying chondromata. There is in many places irregularity of the epiphyseal lines and of the bone growth, but I cannot agree to the suggestion that such cases are in any way connected with true rickets.

Case II.—A case of *Coxa Valga.*—A child, aged four and a half, a patient of Mr. Seton Pringle. Some twelve months ago this girl was healthy, and could walk well. Then she fell into the fire and was severely burnt about the right hip. After prolonged treatment the burn healed, and the patient was found to have both hips ankylosed in a position of partial flexion and abduction. The skiagram shows practically no angle between the shaft and the neck of the femur, the former pointing directly to

he acetabulum. The condition appears to merit the name of oxa valga. Presumably toxic absorption from the burn induced n arthritis, causing ankylosis. My view is that the widening f the angle is not a congenital condition. At the time of the urn the angle was the normally wide angle of a very small hild. Subsequent treatment removed the weight of the body vhich, naturally pressing the femora upwards, makes the angle radually take the adult shape, while the weight of the legs in his case when the child was seated has, I think, in dragging n the ankylosed hips, gradually drawn neck and shaft into ine.

Case III.—A case of *Congenital Dislocation of the Shoulders.*—The patient, a boy of about eighteen, had never noticed anything vrong with his shoulders until a drill serjeant observed that ie could not raise his arms properly above his head. The adiogram shows no proper acetabular cavity. The condition is dentical on both sides. The axillary border of the scapula has n almost vertical position, and extending upwards from the egion of the neck there is an ill-defined articular surface. The ase appears in many respects to closely resemble one described by R. W. Smith in his Treatise on Fractures and Dislocations (1847), p. 268, which he describes as follows :—" I found that there was no trace of glenoid cavity in the natural situation ; but upon he external surface of the neck of the scapula there was a well formed socket, which received the head of the humerus. It was an inch and three-quarters in length and an inch in breadth ; t was a little broader above than below, and its summit was less han a quarter of an inch from the undersurface of the acromion rocess. . . . The axillary margin of the scapula, if pro-onged upwards, would have passed almost altogether internal o this abnormal socket."

In this and the preceding case radiograms were taken in different ngular positions to ensure against fallacies of projection.

Dr. Moorhead inquired if the condition of the neck of the emur was not really a retention of the position that was normal n children. He ventured to doubt the diagnosis that there was sub-acromial dislocation. One could see, he thought, that the humerus was lying directly anterior to the acromion process.

Mr. Wheeler said he considered the hip case to be one of

congenital deformity. The absorption of toxins might account for the ankylosis as in gonorrhœa.

Dr. Hayes said he had read an article on coxa valga some time ago in which several cases, all adults, were described, and the measurements of the different angles were given. He thought the shoulder case showed considerable abnormality in the formation of the scapula.

Dr. Pearson was inclined to agree that the hip case was probably congenital; the deformity was bilateral, but the degree of the deformity shown in the skiagram was greater than could possibly be accounted for by a persistence of the obtuseness of the angle as suggested by Dr. Moorhead. If there was no fault of delivery it was probably a case of true congenital dislocation.

Dr. Harvey, in reply, said he was rather of opinion that it was not a congenital deformity, as no abnormality had been observed prior to the accident, which occurred a year ago, when the child was three years old. At that age the femoral angle would not be so pronounced as in a child of four, but would be considerably more so than the radiogram showed. The burn had become septic, and the child appeared to have been kept a great deal in a sitting position. He took it that toxæmia had produced ankylosis, and that, the weight of the body not being supported by the femora, the angle had ceased to evolve, and had further been straightened out by the pull on the legs in this constrained attitude.

Carcinoma of the Œsophagus.

Dr. Moorhead exhibited a specimen of carcinoma of the œsophagus removed from a female patient who had come into hospital in August last complaining of difficulty in swallowing. He passed a bougie and found obstruction fifteen inches from the teeth. It was noticeable that the second sound in deglutition was absent or delayed. In a screen examination they saw a bismuth mass lodged in the œsophagus above the cardiac end. Shortly afterwards swallowing became almost impossible, and Mr. Stoney performed an operation which, undoubtedly, prolonged the patient's life. On *post mortem* examination the pleura on the left side was adherent, and all the abdominal viscera were

dherent for no apparent reason. The œsophagus was, as had been expected, considerably distended. In the lower end of the œsophagus there was an accumulation of the stones of grapes which the patient had eaten during the last few days of life. The carcinoma was of the usual type. The specimen demonstrated the valvular form of opening which Mr. Stoney had effected.

Carcinoma of Œsophagus with Secondary Deposits.

Dr. Boxwell showed the viscera from a case of carcinoma of the œsophagus followed by abscess at the base of the right lung, with secondary deposits of the growth in the heart, liver, spleen, suprarenals and kidneys. After reading a short clinical history of the case he demonstrated a carcinomatous ulcer at the cardiac end of the œsophagus. This ulcer had perforated into the right pleura, causing an abscess at the base of the right lung. Incorporated in the wall of the abscess was a large branch of the pulmonary vein. There was a small secondary deposit of the growth at the apex of the left ventricle, and the curiously symmetrical deposits in the suprarenals and kidneys seemed to point to a hæmic distribution. There were no secondary deposits in the lungs or bowel.

Mr. Law said he had had the opportunity of trying the œsophagoscope, and had found it generally possible to remove a small piece to put on the microscope. The measurements from the teeth to the obstruction were fairly average, but the distance varied considerably in different patients without much reason, and a margin had to be allowed before they could conclude where the tumour was.

Mr. Gunn said it was very distressing to see such cases die, but he thought the time was not far distant when they would get the correct route, and be able to deal with them surgically.

Dr. Moorhead, in reply to a question by Dr. Cahill, said the patient died of cachexia. They did not use the œsophagoscope, as the diagnosis was obvious.

Gastrectomy for a Rare Condition of the Stomach.

Mr. Wheeler exhibited a stomach removed from a woman who had been sent to the hospital eleven days previously. She had vomited every day for the last year and a half, and

succussion splash was marked. Diagnosis was stricture of the pylorus with dilated stomach. On operating he found not only a stricture at the pylorus, but a tumour of uncertain character on the lesser curvature, involving a deeply excavated ulcer and hard glands. In addition, when he pulled the stomach down further, he found an hour-glass constriction at the cardiac end. There was a very small pouch of stomach above the hour-glass constriction. The glands were found to be non-malignant and the nucleus of the tumour turned out to be a chronic non-malignant ulcer. About three-fourths of the stomach were removed, and anastomosis was effected between the duodenum and the stump of the stomach. The patient had since developed a keen appetite, and was eating solids a week afterwards. She was apparently perfectly well. Bismuth photographs were subsequently taken to show the result. He thought the triple lesion a rare condition.

Gangrenous Intestine following Strangulated Hernia.

Mr. Wheeler also exhibited a specimen of the above. The hernia was of four days standing, and he had to take away about a foot of intestine. The specimen emphasised the necessity of not doing an anastomosis until the intestines had been drained for about a week.

Mr. Gunn said the question of malignancy or non-malignancy was one of great difficulty. He had removed a small portion of the stomach for what he believed to be a malignant ulcer in an elderly man. Dr. Harvey examined it, and said it was not malignant, but after repeated examinations he came on several places with undoubted carcinomatous change in the ulcer. Unless, therefore, a series of sections was made, he thought Mr. Wheeler should be very doubtful as to his case being non-malignant.

Dr. Pearson said that in three hundred odd resections done at Rochester for carcinoma it had been found that in sixty-one per cent. of the cases of cancer of the stomach they could obtain a history indicative of pre-existing chronic ulcer. That set the matter at rest as to whether chronic ulcer predisposed to cancer.

Dr. Moorhead cited a case of his own similar to Mr. Wheeler's in which the hour-glass stricture and the carcinoma were absolutely apart.

Dr. Cahill asked if any estimate had been made of pepsin or any examination for rennet. Where such obstruction occurred, one found rennet present and pepsin diminished.

Dr. Goulding could hardly believe that it was not carcinoma, and thought an exhaustive examination should be made.

Mr. Wheeler, in reply, said his experience was that many such cases were non-malignant. He would have more sections made. Hydrochloric acid was present in small quantities. He believed further sections would also prove the tumour of the stomach non-malignant, notwithstanding the general opinion expressed to the contrary.

Friday, February 4, 1910.

Pyonephrotic Kidney.

Mr. L. G. Gunn, in exhibiting a specimen of pyonephrotic kidney, said the patient had only one of the symptoms commonly associated with the condition—viz., some tenderness in the region of the kidney. He had been sent for on December 12, 1909, to see a lady who apparently had some intestinal obstruction. She had had typhoid fever about eight weeks before. During convalescence she had had two or three attacks of vomiting, and the day before he saw her vomiting had recurred with a rise of temperature, and the vomit was stercoraceous. At the time he thought the case one of either septic infection of the gall-bladder, with adhesions in the hepatic flexure of the colon, or a malignant growth in the large bowel. He treated her with olive oil injections. The tumour subsided, vomiting stopped, and she got very much better. In a few days, however, another attack came on, and he advised an operation. On cutting down on the large bowel they found it pushed out of place by the kidney lying behind it. He freed the bowel to some extent, satisfied himself that there was no definite lesion in it, and then closed the wound, and turned the patient over and removed the kidney. The pelvis was full of pus, and a stone was seen in the kidney. The ureter was normal also. The fact that the urine was normal increased the difficulty of diagnosis. He found it hard to realise that a stone or kink could make the obstruction. The kidney was quite fixed, and he thought the condition in

some of such cases was possibly due to a spasm in the upper part of the ureter. They might, therefore, have to look for a nervous origin in a good many of the cases, and there might be a similar condition present in muscular coats of the bowel causing temporary obstruction.

Ulceration of Larynx following Enteric.

Dr. Day exhibited a specimen of ulceration of the larynx following enteric fever. The patient was a young man, about thirty, who was taken into hospital in advanced enteric fever, and with a bad alcoholic history. He lived only a few days, but shortly before he died he coughed up a good deal of very fœtid mucus which evidently came from his larynx. He exhibited the specimen because of its rarity.

In another case of advanced enteric fever with bad alcoholic history, the patient improved for about ten days, and then got a rigor, for which no cause could be found. They found the liver dulness increased, and on examination they got some bloody fluid with the needle. Afterwards they got some pus, and it was thought he was suffering from empyema. Part of a rib was removed, but only about a teacupful of bloody serum was found and no pus. He sank and died, and they found an infarction in the lower lobe of the right lung.

Professor White said the infarction was very small. From it he got some typhoid bacilli. He also got several kinds of cocci, including a streptococcus, which in its first cultivation was extremely long. His reading of the case was septicæmia following enteric fever. In the specimen of ulceration of the larynx, the ulcer was lower than the usual position. There were no bacilli: the organism in the pus was one of the ordinary staphylococci.

Dr. Parsons recalled a case in which a tracheotomy had to be done on account of the patient's difficulty in breathing. Immediately afterwards surgical emphysema developed, and the swelling spread all over his body right down to the groin. The *post-mortem* revealed a very similar condition to that shown in Dr. Day's first specimen. He asked if the infarct was embolic, and whether he had cardiac signs.

Dr. Goulding referred to a discussion on the subject of

emphysema, at which Dr. Croly, who had performed the operation referred to by Dr. Parsons, said the emphysema was not the result of the operation, but of the condition of the lung.

Dr. Day, in reply, said he had seen three such cases, and in none of them had he seen emphysema. The patient probably died from intense poisoning. The second case had no heart symptoms other than the usual softening associated with bad enteric fever.

Tracheal and Pulmonary Diphtheria.

Dr. Day also exhibited a specimen showing tracheal and pulmonary diphtheria from a girl aged seven years, who had been reported three days ill from "croup." It was evident she was suffering from pulmonary diphtheria, and her condition was so bad that they had to operate. She improved considerably for about thirty hours. Then she got symptoms of further obstruction lower down in the lungs, and died. She coughed up casts of the bronchial tubes. It was peculiar that in spite of 20,000 units of antitoxin the membrane regrew, and had grown over the trachea and down into the bronchial tubes. After the operation the case went through the moist form of the disease, but next day the dry form came, which nearly always terminated fatally. She had one lung almost completely blocked.

Professor White said the larynx was completely blocked with the diphtheritic membrane. The lumen of the trachea was almost completely filled with the exudations. The cut surface of the lung showed most of the bronchi quite filled, though some were partly free. Microscopic sections of the accompanying broncho-pneumonia showed a great tendency to necrosis.

Mr. Gunn inquired as to the method of dosage of the antitoxin.

Dr. W G. Harvey asked if it was possible that the membrane was partially due to some other virulent organism, when it continued to grow after the antitoxin.

Dr. Day, in reply, said they did not go in for large doses, and they apportioned the dose to the severity of the disease rather than to the size of the patient. They gave 4,000 units, and repeated that dose sometimes twice, sometimes three times. He could not say if any other organism was present; they nearly

always diagnosticated diphtheria from the clinical aspects. Swabs in bad cases very often gave negative results. The case was one of the first to show him that they could have diphtheria starting in the lungs and working upwards.

Professor White said that in a recent case which seemed as if an abscess in the tonsil had burst, the culture gave ordinary streptococci. The low temperature, however, still suggested diphtheria. Next day a swab gave an absolutely pure culture of diphtheria bacilli. As regards Dr. Harvey's question of the association of some other virulent organism, he had not investigated that point, as the lung sections were full of diphtheria bacilli.

Sarcoma of the Femur.

Dr. W. G. Harvey exhibited a specimen of sarcoma of the femur taken by amputation from a girl aged fifteen. Three months before admission to hospital she had had a cycle accident. Six weeks later she noticed a swelling on the leg, which gradually increased in size, and at the time of admission was slightly painful, with some increase of local heat. The specimen showed a large swelling at the lower end of the femur which seemed to invade the centre of the bone, but the greater part was sub-periosteal. At first sight microscopically he thought it looked like a carcinoma, but from the point of view of the radiograms, which he showed, it was a typical periosteal sarcoma, and histologically alveolar.

Mr. Gunn said he had looked upon the growth at first as endosteal in origin, but from the photograph he thought it must be periosteal.

Dr. Stokes said there was not much use operating in periosteal sarcoma, as the patients always died.

Professor White said that although the shaft appeared intact he thought the growth originated inside the bone. The section under the microscope was in favour of medullary sarcoma.

Dr. Hayes thought that if the growth originated in the bone there would be greater destruction of the medulla, and some evidence of absorption of the bone: the shadow of the shaft seemed to be perfectly normal.

Dr. A. H. Benson said that if any analogy could be drawn from the action of sarcomata within and without the eye, he agreed

that it was easy to have the walls practically intact. Such sarcomata grew very slowly for a long time inside the eye, but as soon as they got through the sclerotic they grew with great rapidity. The microscopic sections generally determined easily the internal origin of such growths.

Dr. Harvey, in reply, said he called it a periosteal sarcoma more or less from the clinical point of view, but a true periosteal sarcoma, involving only the periosteum, seemed to be exceedingly rare. It was very difficult to determine in which direction the tumour had broken through.

Tricuspid and Mitral Stenosis.

Dr. Moorhead exhibited a heart showing tricuspid and mitral stenosis. The patient, a girl aged twenty-eight, had been admitted to hospital about ten days before suffering from symptoms of severe cardiac failure. On first admission she had a distinct thrill, and a distinct presystolic and systolic murmur, the presystolic being extremely loud over the ensiform cartilage. The presystolic murmur and the thrill both disappeared, and only the systolic murmur persisted. There was a suspicion that she might have, in addition to mitral, a tricuspid stenosis. Later on a double murmur was heard, and he regarded the second as due to aspiration by the right ventricle of blood through a stenosed tricuspid orifice in its diastole. The heart was found to weigh eighteen ounces. On the right side it showed enormous dilatation and hypertrophy of the right auricle. The terminal muscle stood out in a very striking way. Marked tricuspid stenosis was shown: at the *post-mortem* he could get only one finger through the orifice, the right ventricle was dilated and hypertrophied. On the left side there was a good deal of dilatation and hypertrophy of the left auricle, and extreme mitral stenosis was present. The mitral cusps were both calcified and extremely rigid. There was no recognisable aortic murmur, although the aortic valves were also sclerosed. There were some pulmonary infarcts. Both kidneys were extremely irregular on the surface, suggesting congenital lobulation. Looking more closely, he thought the condition might be the result of multiple infarction.

Professor O'Sullivan said it was difficult to decide as to the

condition. He thought there was no doubt as to the presence of arterio-sclerosis. Large depressions extending round a kidney, like a band that had pressed into it, were usually regarded as due to arterio-sclerosis. He looked on the kidney exhibited as one of that type, except that the sclerosis was sporadic.

Dr. Boxwell cited a case of his own which closely resembled the case before them, except that in his case there was a patent foramen ovale suggesting a congenital origin for the tricuspid lesion. The kidneys were very curious. One could not think it arterio-sclerosis of the ordinary kind. There seemed to be a well marked cortex.

Dr. Moorhead, in reply, said that he considered the tricuspid stenosis directly secondary to the mitral, and did not think pre-existing congenital deformity was to be looked for in every case.

Encysted Hæmato-pericardium.

Dr. Boxwell exhibited a specimen of encysted hæmato-pericardium from a man aged thirty-two, who had been sent to hospital in June, 1909, with what was supposed to be an enormously enlarged liver. He came to the conclusion that the liver was not so much enlarged as displaced. The patient gave a history of an attack of influenza in the previous March, during which he had suffered from a pain in his chest. The acute attack passed off in about a fortnight, but during convalescence he noticed a swelling in his abdomen, and he then became progressively weaker. When admitted to hospital early in June he was deeply cyanosed, and could rest only in the sitting posture or leaning forward. The liver dulness extended a hand's breadth below the costal margin, while its upper limit was depressed. He had a left-sided pleural effusion, and a large dull area in the middle of his chest. The heart sounds were rapid and feeble, but there were no murmurs or friction sound. He had no fever. Assuming the displacement of the liver, the diagnosis lay between pericardial effusion and intra-thoracic tumour. The pleural fluid was blood tinged, and this suggested tumour. As the patient was becoming rapidly worse, it was deemed advisable to puncture the pericardium with an exploring needle. This was done in the usual situation, and a syringe full of red blood was drawn off, which clotted, naturally, in a few minutes. Visions of a displaced and punctured auricle

prevented further interference. The patient now developed a loud friction rub on the right side, and in the course of a day or two died. At the *post-mortem* a huge encysted hæmato-pericardium was found; the inner compartment was quite complete, showing a wall of leathery fibrin, with papillary projections on the inner surface, and containing about a quart of red blood. The part of the cyst wall clinging to the ventricle was partially organised. In the outer compartment was a smaller quantity of blood-stained sero-fibrinous fluid. A large hæmorrhage was found in the right lung corresponding to the friction rub. The liver was but little enlarged. The case demonstrated once more the futility of exploring the pericardium with a needle instead of resecting a rib.

Tuberculous Spleen of Pig.

Professor Mettam exhibited a tuberculous spleen of a pig. The normal weight of the spleen was about 140 grammes; the specimen weighed 1,410 grammes. The condition was one which was usually received as being of the nature of Hodgkins' disease, probably because there was always difficulty in finding the bacillus; but that morning he had found a couple of tubercle bacilli in the film.

Friday, March 4, 1910.

The President in the Chair.

Piroplasmosis and Experimental Piroplasmosis.

Professor Mettam read a Paper on above, see p. 343 *ante*.

Anæmic Infarct of the Lung.

Dr. Parsons and Dr. O'Sullivan exhibited a specimen of above.

Professor O'Sullivan said the specimen showed the typical appearance of brown induration, which was not, in his experience, a common thing. Besides the induration there were a number of infarcts scattered through the lung of the ordinary hæmorrhagic kind, but even in those they found a central white patch quite distinct from the rest of the infarction. One showed the way in

which decolourising took place. Another structure presented different characters. On section it showed a small, almost white, mass, which was not very distinctly encapsuled. On examination he noticed a blood vessel which had not been completely cut through. Microscopic sections showed that the whole tissue of the lung was necrosed, and that the capillary vessels were somewhat swollen and contorted and filled with something which could not very well be made out with the stain. All the contorted capillaries took on the fibrin stain, and the substance in the alveoli consisted of distinct threads of fibrin. His reasons for thinking it was an anæmic infarction were, first, its apparent age; an infarct decolourised so completely would be an old infarct; it would have around it a considerable quantity of fibrous tissue forming a definite capsule; secondly, the appearance of the capillaries was different from that found in hæmorrhagic infarction: it was the sort of appearance described as hyaline thrombi in the capillary vessels; thirdly, there was no appearance of blood in the capillary spaces; pigment was found chiefly in the cells, but the lung was pigmented all through.

Professor Thompson asked if any micro-organism had been discovered.

Dr. Kirkpatrick inquired as to the state of the heart.

Professor O'Sullivan said it was an extreme case of mitral stenosis. He was not able to find any micro-organisms, but when tissue died the organisms died with it.

Hypernephroma.

Mr. William Taylor exhibited a specimen of hypernephroma removed from a man fifty years of age. He first saw him in September last, when he got a history of an attack of pneumonia in October or November of 1908. During the interval he had been expectorating quantities of pus every day, with occasional periods of two or three days without expectoration, the expectoration following a pain in his side. He (the patient) had also noticed a swelling in the upper part of his abdomen on the right side. When stripped, a tumour was obvious, extending below the navel to his right iliac fossa. It was hard, but tender on deep pressure, and moved with respiration. He thought the patient had an abscess in the liver, which was being evacuated through the lung.

or a hypophrenic abscess displacing the liver. When the patient went into hospital, he examined him with Dr. Craig, and they agreed it was probably in the right lobe of the liver. He determined to open the abdomen through the right rectus muscle, and on doing so found the liver practically normal. The right lobe was smaller than normal, but it was pushed up, and with it the diaphragm. Then they saw the enormous mass in the kidney. It was plain that if it was the kidney, and communicating with his chest, attempt to remove it would be fatal, so he turned the patient round, and evacuated a considerable quantity of pus with some blood and necrotic tissue. For a couple of days the patient expectorated blood, but this gradually ceased. There was no trace of pus in the sputum after the first ten days. The patient went out and attended to his affairs before they attempted removal of the tumour. Careful inquiry elicited the fact that eighteen years before he passed blood without any apparent cause. During the subsequent three years he had on five or six occasions noticed blood in his urine, but it passed off. About a month ago he was again admitted to hospital in fairly good condition, but discharging fœtid pus freely from the wound. He re-opened the abdomen and removed the growth, which weighed six pounds. He hoped the patient would soon be fit to leave hospital.

Dr. Boxwell said sections showed that it was an epithelial growth. Some parts were solid epithelium and some tubular. It was exceedingly vascular, and seemed to be a typical hypernephroma. Sections of the kidney showed nothing remarkable. Any changes were simply due to the pressure of the tumour and atrophy resulting from it.

Dr. O'Farrell asked if Dr. Boxwell considered such hypernephroma to be adrenal rests or not. They were looked on as a form of endothelioma. He had seen a case which had no necrotic centre ; the whole kidney was removed, and the tumour was found to extend in the pelvis right down to the lower pole. Hypernephroma was likely to form secondary deposits in the bone.

Professor Mettam said the sections looked like adrenal tissue. It was not uncommon to find the condition in cattle frequently incorporated in the liver with the structure of the adrenal type, and resembling the zone of the cortex of the adrenal.

Dr. Boxwell replied that the tumour was encapsulated, but it was hard to say with certainty whether it should be looked on as an adrenal rest. He had failed to find evidence of fat in the cells ; and some parts of the tumour had a delusive resemblance to kidney tissue.

Mr. Taylor, in reply, said an X-ray photograph had been taken by Dr. Hayes before the operation, but no information was forthcoming, except the presence of an enormous dense mass in the right flank.

Abscess of Heart.

Dr. Boxwell showed a specimen of abscess of the heart. The patient from whom the heart was removed was a male, aged forty-five. He had been found leaning against the railings of the hospital, and was admitted in a fainting condition. A hurried superficial examination revealed nothing but an exceedingly fast and weak heart's action. Subsequently, some left basic pneumonia of a week's standing was discovered. His temperature was 100° F., and his pulse anything between 180 and 200. He had a bad alcoholic history, but denied syphilis. Large hypodermic doses of strychnin and digitalin to steady the heart had no effect whatever. He became rapidly weaker, and died next day. At the *post-mortem* an abscess was found in the posterior wall of the left ventricle, discharging pus into the cavity. Section showed the pus to be coming from a large hæmorrhagic and necrotic area occupying the upper third of the left ventricular wall, and extending into the septum. Microscopically one could see what were apparently small abscesses surrounded by necrotic muscular tissue, areas of recent cell infiltration, and a great deal of old standing fibrosis. It occurred to him that there was probably a gumma of the heart wall in the first instance, which had become secondarily infected. A large cirrhotic and nutmeg liver was also found, and a large spleen closely adherent to the diaphragm. There was a small basic pneumonia of the left lung. In the pus taken directly from the abscess a few diplocci and one or two bacilli were found. At least two bacilli and various cocci grew in cultures, but one could not be certain how much was due to *post-mortem* contamination. He had failed to find any organisms in the tissue stained by Gram.

Dr. Kirkpatrick asked if there was any evidence of syphilis in the patient other than the supposed gumma.

Professor O'Sullivan said from the sections shown the condition was almost certainly syphilitic.

Dr. Boxwell, in reply, said he did not find any other evidence of syphilis. The arteries were not materially diseased.

Charcot-Leyden Crystals.

Dr. Parsons exhibited a specimen of sputum from an asthmatic case, in which Charcot-Leyden crystals were present in very large numbers; they were so abundant in some parts that they looked like pure cultures.

Professor Thompson suggested that an attempt should be made to isolate the crystals so as to get to know more about them; very little was known about their chemical composition.

Friday, April 8, 1910.

Dr. Rowlette read the Pathological Report of the Rotunda Hospital. See p. 360, *ante*.

Intra-ocular Sarcoma.

Dr. A. H. Benson exhibited specimens of intra-ocular sarcoma. Intra-ocular sarcomata were divided, according to the stage of development, into four stages for clinical purposes. The first condition was where a man simply began to lose his sight. The cause of the loss of sight was not so much the size of the tumour as the amount of detachment of the retina which the tumour produced. Later on the tumour produced complete detachment, the tension rose, the lens became opaque, and in that stage it was difficult to differentiate from glaucoma. The third stage was where the sarcoma protruded through the cornea, and the fourth stage was where secondary involvement took place.

Mr. Mooney also exhibited similar specimens. Ciliary sarcoma, he said, was not very common. In five years they had got only two at the Eye Hospital, and he had one from the Mater Hospital from Dr. Werner.

Dr. Kirkpatrick complimented Mr. Mooney on the manner in which he had mounted the specimens.

Dr. Boxwell inquired as to how the shape of the eye ball was preserved.

Mr. Mooney, in reply, said he put the eyes in 10 per cent. formalin for twenty-four hours, and then froze and cut them. The cavity of the eye in each case was filled with weak formalin.

Two Brain Tumours.

Mr. William Taylor exhibited two brain tumours, and read the following clinical notes :—

Case I.—The patient, a young woman, about thirty years of age, gave a history of deafness dating back for two years, without, however, vertigo or tinnitus. This symptom was followed later by gradual paralysis of the right facial nerve, and she consulted a doctor, as her face was "getting crooked." In October last she first began to suffer from headacke, which, though intermittent, was often very severe. In January vomiting began; it was of a cerebral type, and continued on and off up to the date of the operation. She then began to suffer from "reeling," mainly in the left side, and it was this symptom which caused her to abandon her occupation as a "general" servant. On admission signs of right seventh nerve paralysis were evident. She was absolutely deaf in her right ear, both to air and bone conduction (eighth nerve). There was some drooping of the right half of the soft palate (eleventh nerve). She was also rather hoarse. She had lost the sense of pain on the right cheek and right side of the tongue. The sense of taste was gone on the right side, and she could not distinguish sweet from bitter (involvement of the fifth and "chorda"). She sometimes noticed excessive salivation and dribbling from her mouth. There was a certain amount of protrusion of the right eye, but this was not constant. There was marked nystagmus, the twitching being much more frequent and finer when the eyes were directed to the left than to the right side. There was internal stabismus of the right eye (paresis of sixth nerve). She was able to distinguish colours almost equally well on both sides, and the peripheral field of vision was good. The eyes reacted well to light. Romberg's sign was absent, and the condition of the jerks variable. There was no tremor, and co-ordination was excellent.

Case II.—A young man, aged twenty-two years; was transferred from Cork Street Fever Hospital to the Meath with the following history:—"He has suffered from Potts Caries in the dorsi-lumbar region; has an atrophied right leg, and a false joint below the knee. He has had a discharge from his left ear for the last ten years, but has no tenderness of the head on percussion. He complains of intense frontal headache. There is no squint. He is dull and drowsy; his temperature is normal, and his pulse 60. His sight is bad. The left eye is blind, and there is a 'choked disc' on the right side. No tubercles on the choroid." A subsequent note reads:—"There has been no marked rise of temperature. His pulse is uniformly slow, and has been as low as 48. Cerebration is good; he speaks quickly and decidedly."

Mr. Taylor continuing, said.—The patient in the first case had been treated outside. She had then gone into the South Dublin Union, and subsequently was transferred to the Meath Hospital under Dr. Craig, who put her on iodide of potassium, but without benefit. She was then transferred to him with a view to operation. He tried to remove the tumour, adopting Horsley's method of dividing the procedure into two stages, first removing nearly the whole of the occipital bone to give complete freedom for dislocation of the cerebellum, and then removing the tumour later. The patient seemed to do very well, but on the second evening she developed pneumonia, and died next day. It was not a cerebello-pontine tumour, as they thought. It was apparently a tumour primarily pontine, and it would have been impossible to remove it without killing the patient.

In the second case, as it was obvious the patient would die unless something was done, he trephined him, but failed to discover anything. On account of the high tension in the skull he did not close the dura mater. The patient died a fortnight afterwards, and at the *post mortem* they found the tumour in the left occipital lobe. It was probably some kind of sarcoma, but had not as yet been microscopically examined. If they had been able to see the patient earlier and get a clearer history they might have been able to localise the tumour, and possibly remove it.

Dr. A. H. Benson said the fact that in the second case there was

neuritis on the right side, while the tumour was found on the left, was in direct contradiction to Victor Horsley's statement.

Dr. Moore said he had had a case of cerebellar tumour in the right side, in which the symptoms commenced in the opposite side, though they spread to the other side in time.

Dr. Kirkpatrick asked if there was any marked relief of the symptoms in the second case as a result of the opening of the skull. He thought that even where nothing could be done in the way of removal, relief from pressure could sometimes be given by operation.

Dr. Boxwell asked if there was any recent explanation of optic neuritis : was it a mere passive congestion, or was it inflammation proper ?

Mr. Taylor, in reply, said there was no relief in the second case, as the patient lay in a semi-unconscious condition until he died, and the decompression was not sufficiently great to give much relief. He had been under the impression that optic neuritis, so-called, was not a true inflammation, but a mere passive exudation. He had been careful to use the term "choked disc," purposely avoiding the misleading term neuritis.

SECTION OF STATE MEDICINE.

THE PRECIPITIN TEST IN MEDICO-LEGAL WORK.

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[Presidential Address given to the State Medicine Section, Jan. 28, 1910.]

1. *Nature of Problem.*

In medico-legal work it is often of great importance to ascertain the source of a blood-stain—the species of animal that furnished the blood of which the stain is composed. For example, in murder cases, the defence often alleges that blood-stains found on the clothes of a suspected person, or on instruments in his possession or traced to him, are due to the blood of some lower animal with which he may have come in contact, perhaps in sport, or in the exercise of a butcher's trade. Until the method of which I now desire to speak came into use the difficulties in the solution of the problem as to the origin of a given blood-stain may be described as practically insuperable. It must be borne in mind that, in medico-legal work, where the life of a fellow-man hangs in the balance, nothing short of absolute certainty ought to suffice, and the only method hitherto available—the microscopic appearance of the blood corpuscles—falls far short of yielding the absolute certainty required. The stain when submitted for examination is practically always dry, and been so for days or weeks, not infrequently for months or years. The erythrocytes have become fused together, their outlines indistinct and distorted, their substance closely adherent to, or entangled in, the stained material.

In this country the mammals, the blood of which has most usually to be distinguished from that of man—viz., the ox, horse, sheep, pig, goat, and rabbit—possess red corpuscles differing so slightly from those of man that only a skilled microscopist, well versed in microscopic work, could hope to distinguish them with certainty from those of man in the perfectly fresh condition. There is no difference in shape, and the size varies within the narrow limits of a few microns. The red corpuscles of man average 7.7. μ, those of the horse 5.6, of the ox 5.6, of the sheep 4.5 to 5.0, of the goat 4.25, of the pig 4.6 to 6.0, and of the rabbit 7.0 to 7.5. In the fresh preparation, such differences are, of course, quite appreciable, and may be relied upon, when a sufficient number of unaltered corpuscles of each kind are available, so that a well-grounded average value may be determined. But how different is the case in medico-legal work!

Speaking from personal experience of many cases which I have examined I can say that it is extremely difficult to obtain from stains dried on fabrics or solid surfaces isolated corpuscles in a condition satisfactory for micrometry. Much depends on the fluid used for isolating the corpuscular elements. A large number of formulas have been suggested, of which the best that I have tried is a 32 per cent. solution of caustic soda or potash. But even careful maceration in this fluid yields so many forms, deformed in shape and differing widely in size, that even with stains of known origin it is difficult to convince oneself that any reliable distinction can be drawn by the aid of the microscope—and this, too, in stains that have only been quite a short time dried—a few hours in the incubator.

It is a commonplace of the books that the circular non-nucleated erythrocytes of the domestic mammals can be readily distinguished from the larger, elliptical, nucleated, erythrocytes of birds, reptiles, and fishes—and a mere glance

suffices to tell the difference *in the fresh preparation.* But, when we come to the conditions that prevail in actual practice, the distinction is far from being such an easy matter as would at first sight appear. Shape and size may be veiled by distortion, nuclei when present may disappear or become hard to recognise—when present, may be simulated by highly refractive granules or dark central areas. Attempts to wash out the stains with water may have produced hæmolysis. From many blood-stains I have failed to isolate any red discs that could be relied upon to supply an answer to the question—Was this blood mammalian ? much to the surprise of jurists who had been led by the statements in the books to suppose that so much at least could be certainly ascertained and deposed to on oath.

We have advanced a long way since 1900, when the diagnosis of mammalian blood was the highest limit of certainty obtainable by the medical jurist. Two cases that have lately occurred in my own experience may serve as examples of what can be accomplished, thanks to the method of sero-diagnosis which I am about to describe. The clothes of a man who was accused of committing a murder having been submitted to me for examination, I found on one article only, the cap, a stain of blood. It was about the size of a threepenny-piece, and appeared to be a single drop which had fallen on the cap from above, and dried on the cloth undisturbed. The stain had evidently been there a considerable time, for, on microscopic examination of scrapings macerated in 32 per cent. soda the red corpuscles appeared so shrivelled and distorted that I could come to no conclusion as to their origin, merely noting that they were of mammalian character and seemed a little small for those of human blood. It did not, however, occur to me at the time that they were of other than human origin. On applying the precipitin test I found that they gave the reaction characteristic not of human, but of horse blood. This

result I duly reported, giving it as my opinion that the blood was of equine origin. On subsequent inquiry it turned out that the accused man was in the employment of a large horse-dealer, and was frequently engaged in assisting at operations performed on horses, more especially in the region of the mouth ; so that a drop of horse blood might readily have fallen unobserved on to his cap and dried there.

The second case was that of a man accused of a particularly brutal murder. On his " leggings " and trousers I detected a number of blood-stains, also on a knife which was found concealed in moist earth and was conclusively traced to the prisoner. A witness who remarked the appearance of the prisoner's knife, when he took it out to cut some twigs for a broom, was told by him that he had been killing a goat. From each of the articles mentioned I succeeded in obtaining a sufficiency of extract for the application of the test, and at once obtained the typical precipitation reaction with human antiserum, and a negative result with the other anti-sera against which I tested it. The human origin of the stain was thus proved. The murderer, whilst awaiting trial, confessed his crime, and has since suffered the extreme penalty of the law.

2. *General Nature of Precipitin Test.*

I will now briefly state how this valuable test originated. Its history will explain how it was that I came to take it up. Precipitins belong to the class of specific anti-substances or immune bodies. They were discovered by bacteriologists during the investigation of the blood serum upon which depends immunity against infective disease. It was whilst following in my own laboratory the results that had been achieved by Uhlenhuth (1), Wassermann and Schütze (2), and by Nuttall (3) in Cambridge that I was enabled to convince myself of the objective reality of the phenomena in question and of their ready applicability in practice. Having

tried the precipitin method in various ways and with every conceivable control, and having mastered the technique (which requires a good deal of time and perseverance), I brought the matter under the notice of the Irish Government in 1902, and have been entrusted by them with the conduct of such investigations in criminal cases.

During the seven years that have since elapsed I have been many times subjected to cross-examination with reference to the exact nature and reliability of the test. Save on one occasion I have always succeeded in explaining it to the satisfaction of the court. But, incidentally, I have found that even in our own profession there exists a great deal of misconception with regard to this matter. Passages from standard text-books on medical jurisprudence have been again and again quoted against me, and my attention has been called to the absence of any official recognition of the test on the part of the English Home Office. For these reasons I have thought it well to avail myself of the opportunity afforded by this Presidential Address to state in the clearest and least technical language I can command the nature of the test, the technique of its application, and the precautions that have to be taken in order to guard against error. I am writing not for experts or immunity but for those members of my own and the legal professions who, being without special knowledge of the subject, are desirous of knowing what has been and what can be accomplished by the aid of these new tests and how they are to be carried out.

As I have said above, precipitins belong to the class of substances called anti-bodies which arise in the blood and tissue liquids of animals (including man) as the result of the entrance of certain foreign substances into the animal economy. Not every foreign substance is capable of giving rise to an anti-body. In order to do so the introduced substance must possess a certain molecular complexity—must, in fact, be

similar in its general nature to the substances which the cell is in the habit of taking in as food, and for the admission of which it possesses what Ehrlich calls receptors. Extraneous substances of simple molecular constitution which are unlike food, and for which the cell does not possess any receptors, are incapable of giving rise to the formation of anti-bodies. Simple metallic substances, such as arsenic, alkaloidal substances like strychnine, and carbohydrates such as sugar and starch, are therefore incapable of giving rise to immunising anti-bodies, though a certain amount of immunity against poisons like arsenic or morphine can be produced by gradually acclimatising the system to their presence—a totally different matter.

In general terms it may be stated that the introduction into the system of foreign substances of albuminous or proteid nature leads to the formation of an anti-body which is found in the serum of the animal so treated. A substance capable of so acting is called an *antigen*. When antigen is brought into relation with serum containing anti-body a reaction takes place between the two, roughly comparable to that which takes place when an acid is brought in contact with an alkali. The two may be said to combine and neutralise each other. The effect may be at once perceptible to our unaided senses, or it may require to be demonstrated by special methods. The following are amongst the chief kinds of anti-bodies :—(1) antitoxins, (2) precipitins, (3) agglutinins, (4) opsonins, and (5) lysins.

Poisons of complex constitution and derived either from animals (snake and spider poison), from the higher vegetables (abrin, ricin), or from bacteria (diphtheria, tetanus) are the genetic bodies of antitoxins. When a toxin is brought into contact with its antitoxin no visible change is produced, but the combination is non-virulent, owing to the occupation of the haptophore group of the toxin, on Ehrlich's hypothesis.

Agglutinins and opsonins come under the head of anti-

bacterial substances, and so do bacteriolysins. The antigen is the micro-organism, and the effect of bringing antigen and antibody into contact is the agglutination, or phagocytosis in the presence of leucocytes), or solution, of the genetic organism, as the case may be.

Under the head of lysins come not only the bacteriolysins just referred to, but also cytolysins, the genetic body being cells foreign to the animal into the economy of which they are introduced. The most important of these are the hæmolysins, which are evoked by the injection of foreign red-blood cells. The effect of bringing the antibody (hæmolytic serum into contact with the antigen (blood corpuscles of the kind used for injection) is that the cell stroma is ruptured and the hæmoglobin diffuses through the liquid. This "laking" of the blood, as it is called, is the most easily observed of all phenomena, and is extensively used for the detection of antigens and antibodies by the method first suggested by Bordet and Gengou (4), more fully worked out by Wassermann (5) and his colleagues, and now generally known as complement-fixation.

For the present, however, we shall confine ourselves to the antibodies that more immediately concern us—viz., precipitins. The antigen of these is serum-albumin or globulin. Any albuminous tissue fluid, even urine (as was, I believe, first shown by Ruffer), can, when injected into an animal of another species, act as antigen. The effect of the addition of serum containing antibody to liquid containing antigen in solution is the production in the previously clear liquid of a haze or opacity which gradually thickens and condenses into little white flocculi. These gradually sink to the bottom and form a cloudy deposit technically called the *precipitum*.

Like all these substances, the precipitating antigen is *specific* in its action. This means that it will produce the above *only with its own antigen*, not with antigen derived from

any other species of animal, save that which furnished the albumen employed for the injection. This specificity is the keynote of all these reactions, and explains their utility in practice. It is not, however, absolute. An antibody produced by injecting albumen from one species will react with antigen from a closely-allied species, as was pointed out by Nuttall (3) in his admirable researches on blood relationship. For example, precipitins made by injecting human antigen will react with the albumen of the higher apes, sheep antibody will react with goat antigen, rabbit antibody will react with hare antigen, &c. But the reaction is, in comparative experiments with properly diluted material, found less distinct with the allied than with the identical antigen, and in this country, where identification of human blood is chiefly required, this source of error is not of much importance and can readily be eliminated by proper control, as will be explained later on.

Of greater importance is the so-called "mammalian reaction" yielded by certain highly potent antisera by virtue of which their reactions extend to antigens derived from animals rather widely removed biologically from those which furnished the albumen used in the injections. Errors from this source can readily be avoided by employing the antigen in highly dilute solutions (1 in 500–1,000).

3. *Technique.*

The first and most difficult part of the procedure is to obtain the antibody. For this purpose I have always used rabbits. As antigen I have sometimes used human blood obtained, with due precaution against contamination, from the placenta during and after parturition. This is troublesome to obtain, and must be used fresh, as it is seldom uncontaminated, and micro-organisms speedily make their appearance in it even when stored in the refrigerator. Where antigen blood or serum is difficult to obtain, or can only be had at long intervals,

it would be quite possible to dry it over sulphuric acid in a vacuum, or in the incubator soaked into filter-paper. Once dry it can be sterilised at 150° without loss of its antigenic property, and can then be stored indefinitely, and when needed brought into solution with normal saline solution, or if preferred with sterile distilled water to which an equal volume of 1.7 per cent. saline solution is added. What I now invariably employ is ascitic or pleural effusion, or hydrocele fluid obtained as aseptically as possible and stored over chloroform. The chloroform can be got rid of by pipetting some of the clear supernatant serum into a sterile petri-dish and leaving it overnight in the incubator, covered only with sterile filter paper. Blood of domestic animals can, of course, be readily obtained from the slaughter-house.

Some of the serum (3, 4, 5, or 10 c.c.) is injected into a rabbit, either intraperitoneally, or into the marginal vein of the ear, with all due precautions, and the injection repeated at intervals of four or five days until 25 c.c. have been introduced, when the animal may be bled from the ear and the serum tested as to its contents in anti-body. The process of bleeding is much facilitated by wrapping the ear in a piece of lint wrung out of hot water. The veins become dilated and the yield of blood is greatly increased. The blood from the divided vein is taken up into a Wright's pipette or allowed to rise into a small test-tube with an obliquely drawn out capillary end. Should there be obvious evidence of the presence of anti-body, such as the production of a distinct opalescent zone at the line of contact with the antigen serum diluted 1 in 100, then the injections are proceeded with till the rabbit has received 70 to 80 c.c. of antigen. Much smaller quantities are often effective. Ten to 14 days after the last injection the animal is chloroformed and bled either by dividing the vessels of the neck or by quickly opening the thorax, cutting the great vessels and rapidly pipetting off the

blood with which the thorax is filled into a sterile Petri dish which is propped up obliquely in the refrigerator till the serum has separated. The serum is then drawn up into sterile pipettes and sealed off and stored in the refrigerator. In practice it is difficult to avoid contamination by hairs, &c., and the serum will seldom keep at room temperature unless filtered through a small Berkefeld candle or treated with 1/10 of its volume of 5 per cent. phenol. I have found, however (in agreement with Graham Smith and Sanger) (6), that even specimens of antiserum that smelt badly on opening the tube, and the deposit from which contained anærobic spores, gave quite useful precipitating reactions if care were taken not to stir up the sediment. Uhlenhuth (7) found that mould-growths did not interfere with the specific power of the serum. The turbidity due to such growths must be allowed to subside or be got rid of by centrifuging. A plan that seems well worth trying is that recommended by von Eisler (8), of drying the antiserum on strips of black paper—the so-called *Naturpapier*. If only small quantities of antiserum are needed at a time, the animal yielding it can be kept alive for many months and its serum be maintained at a very fair level of potency by an occasional injection of antigen. As cachexia is, however, apt to supervene, and the *titre* has to be re-determined each time, this course is not to be recommended.

The process is tedious and liable to cause disappointment. Some batches of antigen prove toxic. I have had a specimen of pig-serum, freshly collected and to all appearance normal, which killed rabbits in doses as low as 2 c.c. intravenously injected. In other cases cachexia supervenes, more especially if the injections are massive (10 c.c. and more). It is important to make a practice of weighing rabbits at frequent intervals, as, if they waste, their serum is useless. Another frequent source of loss is anaphylaxis (9), which usually occurs at the second injection, especially if it has been a large one. Highly

immunised rabbits, the preparation of which has cost much time and trouble, sometimes die from unknown causes in the night, and their serum is thus lost. In order to avoid disappointment, if it is absolutely necessary to have some highly potent precipitating anti-serum in readiness by a certain date it is well to start preparing three rabbits at least eight or ten weeks beforehand.

Before applying it for medico-legal purposes, it is absolutely essential to ascertain the potency of the antiserum. For this purpose small test-tubes are used, about 5 cm. long by 5 mm. in bore. They are conveniently made fresh each time from glass tubing, and are placed in little racks with holes for half a dozen. These racks I have had specially made by a tinplate worker. With an ordinary drawn out capillary pipette I place in the bottom of each about 0.05 C.C. (3 drops) of the serum to be tested, and superpose on its surface with another pipette about 0.1 C.C. of antigen serum diluted with normal saline solution 10, 50, 100, 500, and 1,000 times. By way of control a second set of tubes is charged with the antiserum and treated in the same way with the serum of some other animal similarly diluted. Both antiserum and antigen should be absolutely clear, and the line of demarcation between them sharply marked. Personally, I have not been troubled by opalescent antisera, but if encountered they should be rejected.

Observation is made from time to time up to an hour, the tubes being left meanwhile at the ordinary temperature of the laboratory. The presence of a distinct line or zone of opalescence at the junction of the fluids indicates a positive result. The appearance is not unlike that produced by the nitric acid test as usually applied to albuminous urine. With a powerful antiserum and a strong solution (1 : 100) of homologous antigen the opalescence gradually spreads through most of the supernatant fluid and resolves itself into flocculi which by the following day will be found to have sunk to the bottom of

the tube. When the antiserum is weak or the antigen very dilute (1 : 10,000–20,000) the zone may require careful scrutiny by a trained eye and suitable oblique illumination against a dark background for its detection.

I have found that antisera giving a distinct zone within an hour with antigen diluted 100-fold are quite strong enough for all practical purposes, though I am aware that stronger ones are considered necessary by the German workers. So completely can the test be controlled in actual practice that even comparatively weak sera, reacting only with 100-fold dilutions of antigen, can be made to yield conclusive results.

Highly potent antiserum when brought in contact with solutions of antigen may (and very often does) give rise to pseudo-reactions—*i.e.*, precipitates with non-homologous serum or stain extract. Errors so arising are to be avoided by using the antigen diluted to somewhere about the limit of the titre of the antiserum—not stronger at any rate than 1 per 1,000, and by careful controls with as many other sorts of antigen as are available in similar dilutions.

Nuttall (10) has suggested and worked out a method of measuring the amount of precipitum formed in unit time, and comparing it in tubes containing the same antiserum with different antigens. This method does not seem to have been widely practised, and it appears to me that differences in the density of the precipitum might readily affect its apparent bulk, unless, indeed, it were concentrated by the centrifuge. Personally, I have not tried the method, and prefer to base my opinion upon the rapidity and distinctness with which the reaction come on in the zone of contact between highly potent antiserum and highly diluted antigen.

4. *Testing the Stain.*

Having obtained a satisfactory antiserum, the next step is to prepare an absolutely clear solution of the stain to be tested.

Before proceeding to do this, however, it is necessary to prove that the stain consists of *blood*. This must be done by the usual methods. I always use the benzidine test (11) first, and should this prove positive confirm it by the demonstration of hæmin crystals and the spectroscopic reactions of the pigment, using a small Browning spectroscope or the Abbé micro-spectroscope, if the amount is small. I always put up a preparation in 32 per cent. soda so as to observe the character of the corpuscles.

One must not lose sight of the fact that the precipitin reaction reveals the presence not merely of blood but of any albuminous substance, such as mucus, pus, semen, milk, or albuminous urine derived from the animal that has provided the antigen used for preparing the antiserum. The necessity for proving that the stain-producing substance is blood therefore remains.

This having been satisfactorily demonstrated, the next thing to do is to prepare a solution of the stain in normal saline solution, and to render it absolutely clear and bright by filtration and the centrifuge. If the amount of material is considerable care must be taken not to use it too strong lest pseudo-reactions should give rise to error. It should be colourless, or nearly so, foam on shaking, and give a slight but distinct reaction with the nitric acid test for albumen. Should the amount of solution available be very small—less than 0.05 cm.—the reaction must be carried out in small pear-shaped or lengthily oval pieces of capillary tubing, the liquids being introduced by means of finely drawn out capillary pipettes.

It is absolutely essential to carry out, side by side, with the actual test, a complete series of controls. For this purpose we need blood-stains of man and the chief domestic animals on various substrata. Sheets of filter-paper are the most convenient of these, but it is well to have the blood dried on

woollen, linen and cotton fabrics, also on leather, wood, and metal. This dried material should be of various dates so that one may select for control a specimen not more recent than that under investigation. In addition to the stains of known origin, one ought also to possess at least one other antiserum made with antigen different to the one under investigation. Thus, if human blood be suspected it is well to have in hand some rabbit serum anti to, say, ox-albumen. For obvious reasons the larger one's stock of these antisera the better, and I now endeavour to keep in stock those reacting with the albumen of the horse, ox, sheep, and pig. Lastly, one needs some serum from a normal non-immunised rabbit.

Two of the little stands are now taken, each containing six small test tubes. Each of the tubes in the first stand now receives 0.05 c.c. anti-human serum. With capillary pipettes about double the amount of one of the following dilute solutions is now added to each respectively :—

Test tube 1 receives extract of known human stain.
,, 2 ,, ,, ,, ox ,,
,, 3 ,, ,, ,, horse ,,
,, 4 ,, ,, ,, sheep ,,
,, 5 ,, ,, ,, pig ,,
,, 6 ,, ,, ,, stain under investigation.

The second stand of tubes is now charged as follows :—

Test tube 1 receives normal rabbit serum about 0.05 c.c.
,, 2 ,, anti-human ,, ,, ,, ,,
,, 3 ,, ,, ox ,, ,, ,, ,,
,, 4 ,, ,, horse ,, ,, ,, ,,
,, 5 ,, ,, sheep ,, ,, ,, ,,
,, 6 ,, ,, pig ,, ,, ,, ,,

About 0.1 c.c. of the extract of suspected stain is now

carefully superposed on each with a capillary pipette as above described.

The tubes are observed in fifteen minutes, and again at the end of an hour. Should the stain be of human origin only Nos. 1 and 6 of the first stand and No. 2 of the second stand should show a positive reaction. The negative results in the remaining tubes of the first stand prove that the antiserum used is specific—*i.e.*, will only react with its own antigen. The negative results in the second stand show that the stain is composed of specific human antigen reacting only with its own antibody.

Should the stain be of non-human origin it will, of course, react in the tube containing the corresponding anti-body in the second series (if there is any such), and the conclusion as to its nature can be confirmed by a proper series of controls.

Let us return now to the two concrete cases mentioned earlier in this paper. In the case of the blood stain on the cap the amount of extract available for the test did not exceed 0.1 c.c. By the capillary tube method, however, I was able to superpose enough of it on the surface of five different anti-sera to assure myself that it reacted powerfully with the anti-horse serum, whilst it proved negative over anti-human, anti-ox, anti-sheep, and anti-pig serum, respectively. In another series of tubes I tested the specificity of the anti-horse serum used by running on to its surface a little slightly dilute stain extract from horse, man, ox, sheep, and pig, respectively, and obtained a reaction in the first tube only. The demonstration of the specific nature of the reaction was thus completed and the origin of the stain determined beyond all possible doubt.

In the other case above referred to, the extract from the several articles submitted at once reacted with anti-human serum, but not with sera anti to the albumen of the ox, sheep, pig, or horse. I had no anti-goat serum on which to try it,

but the anti-human serum I was using gave no reaction with extract of goat-stain, so that the positive result with anti-human serum was conclusive.

5. *Sensitiveness of the Test.*

Coming now to the delicacy of the method, my opinion, based on experience of many cases, is that if the operator has at his disposal a powerful anti-serum and good command of capillary-tube technique, the amount of albumin demonstrable by this test is amazingly small. The chief difficulty, in my experience, is not the smallness of the amount of antigen, but the difficulty of obtaining a satisfactory clear solution. Filtration of such minute quantities of stain extract is, of course, out of the question, and we must only have recourse to prolonged centrifugation at high velocities, which is usually successful.

I have never attempted to ascertain the smallest amount of antigen demonstrable by this means, but Uhlenhuth (12) says that it is easy to demonstrate 1/20,000th gramme, and that Hauser was actually able to demonstrate as little as 1/200,000th part of a gramme by the use of the capillary-tube method due to Carnwath (13).

My original intention was to refer in this address to the other sero-diagnostic method of albumen differentiation, that introduced with another object by Bordet and Gengou (4) and now well known under the name of complement-fixation. This reaction depends on the property possessed by mixtures containing antigen and the corresponding antibody, of fixing complement, and thus preventing the hæmolysis of sensitised corpuscles. It can be used for the detection of amboceptors (antibodies) in the presence of the corresponding antigen or for the detection of antigen in the presence of the corresponding amboceptor (antibody). It has been used successfully for the detection of infection by known and cultivated micro-organisms

(typhoid, cholera, tubercle) and also when the infecting micro-organism is unknown or uncultivable. Its principal application has hitherto been in the latter class of case, when it often affords the only available method of making the diagnosis. The infection to the recognition of which it has been mainly applied is that of syphilis. Wassermann first succeeded (14) by its means in demonstrating syphilis amboceptors in the humours of tabetics and paralytic dements.

It is to Neisser and Sachs (15) that we owe the introduction of complement-fixation into medico-legal practice. Their work was based upon the results of Moreschi (16), who first showed that complement is fixed by the union of precipitin with its homologous precipitinogen (antigen). Inasmuch as precipitins are generally looked upon as receptors of the second order provided with a precipitating and a haptophore group, and ordinarily act independently of complement, it is not exactly clear at first sight how the fixation takes place. It is certain that amboceptors are produced during immunisation with foreign albumen, and that, during the process of precipitation they unite with their corresponding antigen and throw down or fix complement. Those who desire precise information as to what goes on during the interaction of these specifically opposed substances are referred to Professor R. Muir's brilliant series of researches published in the *Journal of Hygiene*, and now collected under the title of "Studies in Immunity." Be the explanation what it may, the fact remains that complement is fixed by the union of almost inconceivable minute traces of albumen with its corresponding antibody. The quantity of the combining substances is far too small for any precipitate to be visible, yet the fact that the union has taken place may be proved by the absorption of the complement necessary for the re-activation of an inactive hæmolytic system.

I have some little experience of the test, and hope as time

and opportunity offer to accumulate some more and lay it before the Academy on a future occasion. Meanwhile, I will only say that complement-fixation is a very troublesome and time-consuming procedure when carried out with all the proper controls, and without them it has no scientific value. It is much more difficult than the precipitin reaction, and can hardly be carried out satisfactorily in a laboratory where any other work is going on. Moreover, it is excessively delicate, so much so that, as pointed out by Uhlenhuth, even the trace of protein contained in sweat may fix some of the complement. There are other sources of error. Thus, for example, there are present in various fabrics used for clothing, complement-fixing substances (17), the presence of which has to be specially tested for, and this complicates the already over-elaborate technique. This drawback may be got over in some cases by showing that the substances in question are heat-stable, which is not the case with blood, and in others by the use of artificial hæmolytic amboceptor instead of naturally hæmolytic serum as originally recommended. I am inclined to agree with Uhlenhuth when he says (*loc. cit.*) that the delicacy of the precipitin test (which can detect 1/20,000th gramme of foreign albumen) suffices for all practical purposes, and that it must seldom be necessary to invoke the aid of complement-fixation in medico-legal work.

Outside of medico-legal work there have been found several interesting and valuable applications for the precipitin method, which I will, in conclusion, briefly touch upon. There is, for example :—

(*a*) The diagnosis of bacterial infections by adding the serum of the infected animal (antibody) to the culture fluid of the suspected organism, filtered free from the bacterial bodies, but containing their soluble extract or metabolic products (antigen). If the two correspond, a specific precipitate is produced, the formation or absence of which can be

utilised for diagnostic purposes, as was first, I believe, shown by Wladimiroff in the case of glanders infection of the horse. The reaction has been found to work well in the case of typhoid, cholera and streptococci. This procedure has been very fully studied by R. Kraus, to whom we owe most of our knowledge on the subject (18).

(*b*) The recognition of the species of animal from which almost any given tissue or albuminous fluid has been obtained, not merely blood or serum. Thus, for example, by injecting cow milk serum into rabbits, Fish (19) was enabled to obtain an antiserum (lactoserum) which produced a specific precipitate with the serum of cow-milk, but not with that from other animals, and which could therefore be employed for diagnostic purposes. It would seem that even boiled milk can have its origin so determined. Working on these lines, Sion and Laptes (20) claim to have traced the origin of samples of cheese. Similarly, in the case of egg-albumen, Myers and Uhlenhuth (21) found that specifically different anti-sera are yielded by rabbits injected with the whites of the eggs of different species of birds. In this way it was found possible to determine whether a given sample of egg-white was produced by a common fowl, a duck, a turkey, or a plover.

A further application of this reaction, which has proved of the utmost practical value in meat-inspection, is that whereby the nature of a given sample of muscular tissue can be determined. Rabbits injected, not necessarily with muscle-juice, but with the blood or serum of, say, the horse, dog, or cat, yield antisera, which, when brought in contact with extracts of the muscle-tissue of these animals, give rise to highly specific precipitates. In this way it is possible to ascertain the species of animal from which a given piece of flesh was cut, and to refer the several components of a sausage to their respective origins. The German Public Health Administration has taken advantage of this discovery, and now actually prescribes the

use of the precipitin reaction in cases of suspected importation of forbidden kinds of meat, adulteration of sausages with horse or dog-flesh, &c.

Along these lines it would be possible to determine the origin of the various kinds of meat extract that are now on the market, provided that they had not been heated to such a point as to destroy their antigens. This has actually been accomplished in a few cases, and the claim that the extract was made from beef was substantiated in the case of one brand, whilst in another the only albumen demonstrable was found to be derived from hen's eggs! (22) An interesting and valuable field of work is thus thrown open to the food analyst who is possessed of the necessary biological training and the requisite facilities for animal experiment. Nor is the applicability of the method confined to albumens of animal origin. Antisera have been made (23) whereby the protein of the grains of cereals can be differentiated from that of the legumes.

(*c*) Different *kinds* of protein derived from the *same species* of animal can sometimes be thus differentiated. Hamburger (24) was able to obtain antisera differentiating the albumen from the casein of cow's milk. Uhlenhuth (25), by injecting separately the white and the yolk of hen's eggs into different rabbits was able to obtain antisera specific for each. Of more direct medical interest are the experiments of Weichardt (26) and Liepmann (27), who by injecting rabbits with placental syncytium were able (by the method of elective saturation) to obtain a serum capable of precipitating one kind of albumen only—that derived from syncytial cells. From this it would appear to be only a short step to obtain antisera exercising a specific action on cancer cells or the albumen derived from them, supposing (which is not certain) that such cells possess any specific properties not shared by the normal epithelium at their seat of origin. Experiments have been made in this

direction, but so far as I am aware without success (28). In this connection it is interesting to note that by injecting extracts of crystalline lenses from different animals, Uhlenhuth (29) found that the anti-sera so obtained did not react with the serum or extracts of other organs of the animal that provided the lenses used for the injections. Antisera made from the lense of various animals (man, pig, fowl, frog) were found to react equally well with the extract of the lense of any animal. From this it would appear that the lense contains an albumen *sui generis*, specific for the organ, but not for the species, and not sharing the biological properties of the blood-derived albumens of the rest of the body. This is a highly interesting observation when viewed in the light of what we know with regard to the origin of the lense from the embryonic ectodermal epithelium. Römer (30) has made use of it to support his theory that senile cataract is due to cytotoxic influences.

(*d*) Lastly, an interesting controversy has arisen as to whether a precipitin can be obtained from the tissues of Egyptian mummies that have been preserved for thousands of years. The inquiry was led up to by the ascertained fact that the origin of bones that have been buried for various periods can be revealed by the precipitin reaction, provided that an extract containing dissolved albumen can still be obtained from them. As the result of their investigations carried out on Egyptian mummies from 3,000 to 5,000 years old von Hansemann and Meyer maintain that their tissues react to anti-human serum with sufficient distinctness to enable their human origin to be so demonstrated. On the other hand, Uhlenhuth completely failed to obtain from 27 Egyptian and Peruvian mummies which he tested any extract capable of yielding a precipitin reaction. Indeed, he states (31) that he failed even with a mummy only 300 years old. In this negative result he is confirmed by Schmidt, of Cairo (32), who

was able to obtain from a mummy of the prehistoric period (at least 6,000 years old) an extract containing sufficient albumen to give a biuret reaction, but failing to yield a precipitate with human antiserum. The proteid matter would seem to have undergone some radical change during its long period of preservation.

In these latter observations I feel that I have wandered far from my proper subject. But I felt sure that in an opening address, as this, a somewhat general survey of the possibilities of the precipitin method would not be unwelcome to our Section of State Medicine.[a]

[a] A demonstration was then given of the mode of determining the *titre* of a specimen of anti-human rabbit-serum, and a stain of bovine origin was tested with several antisera and referred to its proper source. The stain in question had been dried for two years on boot leather.

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Dr. O'Farrell referred to the work done by Professor Muir in connection with the overlapping of the various serums. He desired to know if Professor McWeeney had tried heating the different serums, and driving off the complement, and seeing if the reaction came about by adding fresh complement. The possible presence in linen of some complement-fixation substance was easily understood, as the material in preparation being macerated for a long time much chemical changes must take place. Was it possible, he asked, that by bringing about a reaction in cancer cells an early diagnosis of carcinoma could be made?

Professor Mettam said he believed he was correct in saying that Nuttall stated that simple frothing contained quite sufficient of protein material to give a reaction with the anti-serum. He did not quite agree with Professor McWeeney on the question of anaphylaxis. He thought it usually arose after the second dose when a major dose was given. In Professor McWeeney's cases the doses given were equivalent. He would like to draw attention to the explanation given by Vaughan. He took egg-white, and found it was composed of two bodies, one soluble in alcohol and one not. When injected, one was poisonous and one was not. The explanation of anaphylaxis was that in the minimum dose the poisonous was split off from the non-poisonous molecule, and the non-poisonous produced sets of anti-bodies, and the poisonous, being of so small a quantity, was harmless to the animal, or did insignificant damage. As a result of the injection of the non-poisonous portion a number of receptors were formed in the body. Then, when a larger quantity came in it was immediately split, and the non-poisonous parts of them were immediately attacked by the receptors, which liberated a large amount of the poisonous element, and this caused death.

Professor McWeeney, in reply, said the presence of acid or alkali, even in small quantities, completely upset the reaction. He had tried to reactivate inactivated precipitin serum with complement, but had failed. Precipitin serum stood heating

better than hæmolytic serum. It was evident that its precipitating group was much more stable than complement. He saw that his solution was quite colourless. It should foam briskly and give a slight, but distinct, nitric acid reaction. It should be worked down by degrees until it ceased to give a reaction. Of course, they were reduced to surmise as to what the actual dilution was. It had been found that serum taken from anaphylactic animals had no complement in it; and, inasmuch as complement was a most important constituent of human serum, it seemed to him more natural to account for the symptoms of the animals by the deprivation of an important element in the blood than by a purely hypothetical splitting of an albuminous substance, which seemed to him to belong to the realm of incompletely ascertained facts. His anaphylactic animals had had first a small dose, and then a second large one; the result was they died inside five minutes.

THE HISTORY OF THE PREVENTION OF SMALL-POX.

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[Read in the Section of State Medicine, April 15, 1910.]

The early history of small-pox is involved in great obscurity, and since the time of Rhazes in the tenth century medical historians have disputed as to both the date and place of its origin. The most generally accepted opinion is that the disease was introduced into Europe by the followers of Mahomet, and tradition dates its origin from the celebrated "War of the Elephant," which took place in the year of the birth of the Prophet, or 569 of our era. The war of the elephant was a religious war of great celebrity in Arabia. Abrahah, an Abyssinian prince and a Christian, built a magnificent church at Sanaa with the idea of attracting to it the Arabian pilgrims from their worship at Mecca. The inhabitants of Mecca secretly defiled this church, and so enraged Abrahah that he determined to destroy the temple at Mecca. In order to effect this purpose he raised a great army, and, mounted on a large elephant, marched on Mecca. The town was in a state of helplessness and its destruction seemed certain, but the story goes that when Abrahah attempted to enter the city the elephant knelt down, then turned round, and refused to advance. At the same time a flock of supernatural birds came flying in from the sea. These birds were black and green in colour, and had white and yellow bills; each of them carried a stone about the size of a pea in its

bill and one in each claw. These stones the birds dropped on the heads of the soldiers of Abrahah's army; the stones pierced the helmets of the soldiers and destroyed the entire army. Other accounts attribute the destruction of the Abyssinian army to a new pestilence, called small-pox, which from that time forward spread over northern Africa, and from there to Europe. In historical times the fate of armies has more than once been decided by disease rather than by battle, and it is quite probable that the defenceless Mecca was saved in this way, and that the story of the birds was afterwards invented for the edification of the faithful. One must remember, however, that in mediæval times the Christians of Europe were not loath to attribute one of the most pestilential of diseases to the malign influence of the followers of the Prophet.

That small-pox first visited the Western Empire about this time is borne out by the fact that the earliest known work on the disease was written by a physician of Alexandria, Ahron, who lived during the lifetime of the Prophet in the early part of the seventh century. No copy of this work is now known to exist, but extracts from it have been preserved for us in the writings of Rhazes.

We have, however, records of small-pox much earlier in both India and China. The Hindoo records indicate the existence of the disease in that country from very early times. There are names for the disease in the ancient Sanscrit, and the goddess of small-pox was worshipped under various names all over the country. Various names are given to this goddess, such as Mariatali and Patragali, and different explanations of her origin and power are given in the Hindoo mythologies. The treatment of patients affected with the disease was left to the goddess, who was invoked by prayers and

offerings, and to the Brahmins of her temple. These priests used a form of inoculation and a certain form of prayer, and the secret of their treatment was hereditary and closely kept. There is evidence that the disease was looked on with much dread, and the power of the goddess was greatly respected. There is a picture of one of the forms of this deity given as a frontispiece in James Moore's history of small-pox. The goddess, a huge figure, stands with a crooked dagger in each hand, uplifted and ready to strike on both sides. Before her stand a body of armed warriors ready to execute her vengeance. The two in the background wear red masks, carry black shields, and brandish naked scimitars. The lines radiating from the others are supposed to indicate the infection. Further off is a group of men with spotted bodies, waving black feathers, and with bells hung to their belts, carrying with them infection and death. The other group, carrying musical instruments, is supposed to be supplicating the mercy of the goddess, while the women behind her carry baskets of offerings on their heads as a thanksgiving for their lives and beauty saved. The small child taking something from the right arm of the goddess is supposed to be emblematic of inoculation.

Small-pox raged for many centuries in India, though in the sixteenth and seventeenth it was said to be so rare on parts of the coast that the Portuguese sailors were accused of introducing it into the country.

In China small-pox has been known from at least the third century B.C., and it was said to have been introduced into that country from Central Asia. Inoculation has been in use from 590 A.D., and is known as "sowing the small-pox," from the custom of introducing crusts from the small-pox pustules into the nose, and so infect-

ing the patient from a favourable case. The Chinese also worship a goddess of small-pox. There is a record of an epidemic of small-pox in Japan in the middle of the eighth century of our era, and Dr. Engelbert Kaempfer, physician to the Dutch Embassy in Japan, writing in 1690, states that both small-pox and measles were then diffused throughout the country.

In Thibet small-pox seems to have been present from very early times, and was greatly feared by the people. Captain Turner, Ambassador to the Tishoo Lama, writing in 1800, points out the very effectual way in which the people of that country dealt with outbreaks of the disease. When a case of small-pox was discovered the strictest isolation was enforced, the house and its inhabitants were shut up and all communication was cut off, even though this might entail the death of the people from starvation, and the house or village was afterwards destroyed. When small-pox broke out in the capital the Tishoo Lama left the place, and it was abandoned for three years without inhabitants till infection was considered to have died out. At the time of Captain Turner's visit it is stated that small-pox was seldom met with in Thibet, and when it did occur its progress was checked by the terror and vigilance of the inhabitants.

The evidence at our disposal would seem to suggest that Central Asia and possibly Central Africa were the original homes of small-pox, and that it has spread thence to the rest of the world. That it did not spread earlier need cause no surprise when we remember the great difficulty in communication between distant countries in those early days, and the time it took to get from one to the other. If infection did occur on board the ships of those early travellers it had ample time to die out before they reached home, and we can well believe

that there was little hesitation in abandoning a patient who was known to suffer from an infectious disease. It is certain at all events that there is no definite mention of the disease in the works of the Greek medical writers that have come down to us. It is possible, though unlikely, that the plague of Athens, described by Herodotus, was the small-pox, and most modern historians believe the disease to have been the true plague. Rhazes attributes a knowledge of the disease to Galen, but in this he was most probably wrong, and even he himself admits that if Galen did know the disease he has left us no information as to its treatment, a most unlikely state of affairs in the case of a voluminous writer such as Galen was. Paulus Ægineta, who is described as the last of the Greeks, and who wrote about 622 A.D., makes no mention of small-pox, though he states he "has left out no disease as far as possible." This is important evidence when we remember that Ahron, writing about the same time at Alexandria, states that the disease was fairly well known there at his time. Medical historians from the time of Freind have generally attributed the introduction of the disease, as well as our knowledge of it, to the Saracens. The first definite mention of the word "variola" occurs in the description of Marius, Bishop of Vaux, in Switzerland, who says that "in the year 570 a violent malady, with relaxations of the bowels and variola, affected Italy and France." It is questionable whether the variola here mentioned is the small-pox as we know it, for Gregory, of Tours, speaking with personal knowledge of this epidemic, uses words which appear to leave no doubt that plague was referred to. It is probable that small-pox did not become at all general, in Northern Europe at all events, till after the tenth century, and it is not unlikely that the spread of the disease

was in a great measure assisted by the crusades. In England and Ireland the early references to the disease are very vague. In the Anglo-Saxon Leech Book of Bald, written about the middle of the tenth century, there is reference to the "Pock," but that this refers to the small-pox is more than doubtful. The author says:—"Against the pocks a man shall freely employ blood-letting and drink melted butter, a bowl full of it: if they break out one must delve away each one of them with a thorn; and then let him drip wine or alder drink within them, then they will not be seen, or no trace will remain." Gilbert, one of the first of the English writers on Medicine, refers to small-pox, but he merely repeats the teaching of the Arabians. John of Gaddesden, who, in the early part of the fourteenth century, wrote the celebrated *Rosa Anglica*, repeats the description of small-pox that was given by the Arabian writers, but he is remarkable in that he states that he treated King Edward's son, the Prince, afterwards Edward II., for small-pox, and cured him by the use of red clothes, so that not a mark was left. This treatment was known in Japan from very early times. Holinshed, in his Chronicle, published in 1577, writing of the year 1366, states that "also manie died of the small-pox, both men, women, and children." It is probable, however, that this information is taken from an earlier chronicle in which the word "pokkes" is used, in a generic sense, for any skin lesion. Creighton, who has made very full investigations into the early history of small-pox in England, can find no mention of the disease in contemporary letters before the beginning of the sixteenth century. In 1514 it was stated that Henry VIII. had been ill of a disease which the physicians feared would turn to small-pox, but the King was recovered. There is among the Harleian manuscripts a letter dated

May 11, 1528, which states that some of the Royal Princesses are sick of the small-pox. It would seem probable that it was not till the end of the sixteenth or the beginning of the seventeenth century that small-pox became established in England.

Our information as to the time when small-pox was introduced into Ireland is also very conflicting. We have no definite information as to epidemics of the disease before the beginning of the eighteenth century, but Wilde states that it is referred to in the Irish MSS. from the beginning of the fifteenth. Dr. Rogers, when describing the epidemic of small-pox as it appeared in the South of Ireland at the beginning of the eighteenth century, says:—"This distemper, though of foreign growth and by transplantation brought amongst us, is now become a weed of our own soil and a native of our own country. It is well known that it came from Arabia, and that it can claim no longer a descent with us than about two centuries." "This," Wilde remarks, "well accords with the date of the first MSS. in which it is mentioned." Describing the epidemic of 1708 at Cork, Rogers says that it was "of the most crude and worst kind, that swept away multitudes."

From this view of the spread of small-pox let us now turn to consider the measures that were adopted to check its ravages and to cure the patients affected with it. In Thibet we have seen that a very strict isolation was enforced, and that the people of that country held the life of the individual as of little moment compared with the health of the community. In India and China it is probable that similar ideas prevailed to some extent, for we know how little value, even at the present time, is placed on the life of the individual in those countries. We have seen also how the help of the gods was sought

by prayers and offerings to check the disease, and how this help was supplemented by the treatment of the priests and by the practice of inoculation. It is probable also that considerable knowledge of the medical treatment of the disease existed in India in very early times, for Rhazes tells us of a certain syrup of pearls that the Indians use, of which they say that if anyone drinks of it "though nine pustules have already come out, there will not appear a tenth." The Greeks have left us nothing concerning the treatment of small-pox, probably because they were not acquainted with the disease, and our first definite medical pronouncement concerning treatment comes from the Arabian physicians. This treatment was purely personal, directed towards the cure of the individual, or the rendering of the individual less liable to attack, or better able to resist the disease should the attack come, and in no sense was it attempted to deal with the disease as it affects the community as does modern Preventive Medicine.

Rhazes, the first medical writer on small-pox whose works have come down to us, believed that fermentation and ebullition of the blood caused the disease, and as the blood of children tends much more to ebullition than the blood of old people, very few children escape small-pox, and old people rarely get it. As a preventive he recommends bleeding, cold bathing, and a strict regimen to keep the ebullition of the blood in check, and this, he says, "with God's permission, will have a good effect." In curative treatment he has much sound advice to give. He warns physicians to have great care lest they offend against nature, especially in depriving the body of its natural heat lest one does damage by "depressing the power which the natural heat has of resisting what is hostile to itself." He recommends great care to be taken

of the eyes to prevent blindness, and has many prescriptions both for this and for dealing with the pustules in their various stages. The followers of Rhazes for many hundreds of years advanced little on his teaching and methods. Avicenna (A.D. 992-1050) recommended that the pustules should be opened with golden needles, and this practice was still advocated in the sixteenth century. Averrhoes, a Spanish physician of the twelfth century, believed that the properties of substances depended greatly on their colour, and as "all red colours were hot from the fiery particles with which they manifestly abounded" red coloured things should be used about the patient to help in the maturation of the pock. Gilbert, an English physician of the reign of Edward I., further developed this idea by insisting that the patient should even have red coloured drinks, and we have seen that Gaddesden, his successor, boldly claimed that in this way he had cured the King's son. The important thing was that the patient should be kept hot in order to draw the pustules to the surface, and that he should be blooded to remove as far as possible the evil from his body. The first great change in the treatment of small-pox patients was introduced by Thomas Sydenham, called the father of English medicine, who was born in the year 1624, and died in 1689. He was more cautious in the use of bleeding, and tried to make his patient comfortable. Thus we find him recommending cooling drinks and plenty of fresh air, while at the same time he banished the multitudes of red blankets. He gives a very accurate description of the disease, and looked on the discrete form as one "in no wise dangerous in itself." He goes on to say that—"From these statements it is easy to answer the common question, as to why so many of the poor survive, and so many of the rich sink under an attack of small-pox; that

is, comparatively speaking. This can be referred to one cause only—viz., the want of opportunity on the part of the poor man for treating himself by a nice and delicate regimen. Their *res angusta domi*, as well as their more countrified manner of life, ensures this." Sydenham has nothing to say of preventive measures; he believed the disease to be the result of a "variolous constitution," and suggests its dependence on telluric and atmospheric conditions, over which we have no control. The practice of Sydenham, though bitterly opposed during his lifetime, gained great support afterwards, and continued to regulate English practice till recent years.

The first great change that was made in the treatment of small-pox in Western Europe after the time of Sydenham was the introduction, early in the eighteenth century, of inoculation. We have seen that there is reason to believe that this was practised both in China and India from early times. In India a particular sect of Brahmins travelled through the provinces and performed with much religious ceremony this operation. Persons about to be inoculated were advised to abstain from the use of both milk and butter for at least a month previously. Men were usually inoculated on the arms, and women low down on the shoulders. The skin of the part where the inoculation was to be made was first rubbed with a cloth, and often it was one of considerable value; this cloth afterwards became the property of the Brahmin. A few scratches were then made on the skin by some sharp instrument, and over these was bandaged a small pledget of wool that had been soaked in some variolous pus at least a year before. The wool was first moistened in some holy water from the Ganges, and during the operation the Brahmin repeated prayers from the Attharva Veda. In six hours the bandage was re-

moved and the wool permitted to fall off of itself. Next morning cold water was poured on the head and shoulders of the patient, and this treatment was repeated every day till the fever appeared. The bathing was then interrupted till the eruption came out. When this appeared the bathing was begun again, and persisted in twice a day till the crusts fell off. When the pustules began to change colour they were opened by a sharp thorn. During the entire period of sickness the patient was to remain out of doors, though sometimes he was permitted to lie on a mat at the door of the house during the height of the fever. Cooling food and drinks were given during the illness, and the patient was directed to offer his prayers to the goddess for his recovery, while a present to the operator was expected as a thank offering to the goddess.

A similar practice came into use in other countries, though we have no information as to its origin. The operation was often performed by old women, and several punctures were made in different parts of the body. This was spoken of as "buying the small-pox," as the child to be inoculated was expected to bring the patient from whom the infection was taken a present of some dates or raisins as the price of the variolous matter. In certain parts of England and Scotland also this practice was in use among the poor people. Inoculation, however, as practised in Western Europe differed from that in India in that there was little care taken in the preparation of the patient, and recent infection was generally used. At the beginning of the eighteenth century inoculation was practised with great success in Constantinople, and in the year 1713 Dr. Emanuel Timoni, a Greek resident in that city, who had graduated in Oxford, wrote an account of the practice to Dr. Woodward, of England. This account was published in the "Philosophical Transactions

of the Royal Society of London" during the next year. In 1715 the Venetian Consul at Smyrna published in Venice an account of the Turkish practice. The accounts were corroborated by an English surgeon, Mr. Kennedy, who had travelled in Turkey. Mr. Kennedy published a paper on the subject in 1715, and in it speaks of "engrafting the small-pox." These accounts did not attract any attention from the medical profession in England at the time. Dr. Timoni states in his paper that the practice had been in use in Constantinople for over forty years, and that he himself had personal experience of it for eight years, but he did not know of any patient who had died of the inoculated disease. It is due, however, to the action of Lady Mary Wortley Montague that the practice of inoculation was introduced into England.

In 1716 Lady Mary had accompanied her husband to Constantinople, where he had been appointed ambassador, and from that city many of her celebrated letters were written, giving an account of Turkish customs, and one of these, dated "Adrianople, April 1, O. S., 1718," runs as follows:—"*Apropos* of distempers, I am going to tell you of a thing that will make you wish yourself here. The small-pox, so fatal, and so general amongst us, is here entirely harmless by the invention of *ingrafting*, which is the term they give it. There is a set of old women who make it their business to perform the operation every autumn, in the month of September, when the great heat is abated. People send to one another to know if any of their family has a mind to have small-pox: they make parties for this purpose, and when they are met (commonly fifteen or sixteen together), the old woman comes with a nut-shell full of the matter of the best sort of small-pox, and asks what vein you please to have opened. She immediately rips open that you offer to her

with a large needle (which gives you no more pain than a common scratch), and puts into the vein as much matter as can lye upon the head of her needle, and after that binds up the little wound with a hollow bit of shell; and in this manner opens four or five veins. The Grecians have commonly the superstition of opening one in the middle of the forehead, one in each arm, and one on the breast, to mark the sign of the cross; but this has a very ill effect, all these wounds leaving little scars, and is not done by those who are not superstitious, who choose to have them in the legs, or that part of the arm that is concealed. The children or young patients play together all the rest of the day, and are in perfect health to the eighth. Then the fever begins to seize them, and they keep their beds two days, very seldom three. They have rarely above twenty or thirty in their faces, which never mark; and in eight days' time they are as well as before their illness. Where they are wounded, there remain running sores during the distemper, which I don't doubt is a great relief to it. Every year thousands undergo this operation; and the French ambassador says pleasantly, that they take the small-pox here by way of diversion, as they take the waters in other countries. There is no example of any one that has died of it; and you may believe I am well satisfied of the safety of this experiment, since I intend to try it on my dear little son.

"I am patriot enough to take pains to bring this useful invention into fashion in England; and I should not fail to write to some of our doctors very particularly about it, if I knew any one of them that I thought had virtue to destroy such a considerable branch of their revenue for the good of mankind. But that distemper is too beneficial to them not to expose to all their resentment the hardy wight that should undertake to put an end to it.

Perhaps, if I live to return, I may, however, have courage to war with them. Upon this occasion admire the heroism in the heart of your friend, &c."

Lady Mary was as good as her word. Writing to her husband, "Sunday, March 23, 1719," she says—"The boy was engrafted last Tuesday, and is at this time singing and playing, very impatient for his supper: I pray God my next may give as good an account of him. I cannot engraft the girl; for her nurse has not had the small-pox." This child, at that time three years of age, made a satisfactory recovery. On her return to England in 1722 Lady Mary decided to have her daughter, afterwards Lady Bute, inoculated, and sent for her surgeon, Mr. Maitland, who had attended her son at Constantinople, and desired him to perform the operation. Maitland tried to dissuade her, and suggested a consultation, but this was refused, and eventually Maitland inoculated the child. During the subsequent illness the child was watched by an old family apothecary and three physicians. It is stated that Lady Mary was so distrustful of these physicians that she never cared to leave the child when they were present. The inoculation was so satisfactory that Dr. Keith, one of the attending physicians, got Maitland to inoculate his own child. The report of these cases spread through the town, but the profession was greatly against the practice, and no more inoculations were done for some time. Caroline, Princess of Wales, who had already nearly lost one of her daughters by the small-pox, was most anxious to have the others inoculated. In order to test the method she obtained leave from George I. to have six condemned felons pardoned for the good of the public provided they would submit to inoculation. The felons were willing, but Maitland refused to inoculate them, fearing the

odium that would follow an unsatisfactory result. Sir Hans Sloan, then Court Physician, was appealed to in the difficulty, and when he, after consultation with a Dr. Terry, who had practised in Turkey, advised the trial of the experiment, Maitland consented, and inoculated six felons. Five of them contracted small-pox in a mild degree, the sixth escaped, but afterwards admitted having had small-pox in infancy. All six, however, escaped hanging. Orders were then given for the inoculation of the children on the charity of St. James' Parish, and this was done in the case of eleven of them, with a most successful result. The Princess was now satisfied, and determined to proceed with the inoculation of her own daughters. The King had a consultation with Sir Hans Sloan, and as a result of this granted his permission. The Serjeant-Surgeon inoculated the two children, and the resulting small-pox ran a most satisfactory course. The Royal example was followed to a considerable extent, but the profession and many of the clergy were greatly against it, and we are told that during the first eight years only 845 persons were inoculated, and of these 17 died. In other places the mortality was considerable, and such a clamour was raised against it that some States prohibited inoculation by law. People were told that if they died of natural small-pox it was an act of God, but if they died of inoculated small-pox they would be looked on as having committed suicide, and parents of those children who died after inoculation would be considered guilty of infanticide. The practice was said to be the invention of Satan and the offspring of atheism. In spite of many statistics published to show that the inoculated small-pox was much less fatal to the individual than the natural, inoculation made little headway. Thus Dr. Jurin stated " that of all the children that are born, there

will sometime or other die of small-pox one in fourteen," and "that of persons of all ages taken ill of the natural small-pox there will die of that distemper one in five or six," yet only one in sixty of the inoculated had died. In 1747 a small-pox hospital for the poor was started in London, and inoculation was practised there with satisfactory results. At first the inoculated patients were confined to the hospital till all danger of infection was passed, but later on all persons who applied at the gates were inoculated, and so allowed to carry the infection broadcast through the town. In 1752 some 3,538 persons died of small-pox in London alone. In the year 1754 the College of Physicians in London published a strong approbation of the practice of inoculation, and subsequently the medical opposition considerably declined.

For some time it was noticed that inoculation as practised by the medical profession was much more fatal than it was in the hands of monks and old women, and this gave the opportunity to Daniel Sutton, with whose name the practice of inoculation for the next few years was intimately associated. Daniel Sutton was the son of Robert Sutton, a surgeon at Debenham, and there for a time he and his brother assisted their father. Daniel then served as an Assistant Surgeon at Oxford, but returned to his father in 1763. He then proposed some modifications in the practice of inoculation which his father condemned as dangerous. This led to a disagreement between them, and Daniel settled for himself in Ingatestone, in Essex, where he set up as an inoculator, and claimed to be possessed of a secret that was infallible. By means of advertisements Sutton soon gained great notoriety, and it is said that he hired a clergyman to preach in his favour. This clergyman stated that Sutton and his assistants had inoculated over 2,000 persons with-

out fairly losing one. Many people sought the secret of his success, and it was his habit to communicate it to practitioners on condition of receiving a share of their profits. Finally, in old age, Sutton published his system, and it was found to contain nothing that was not previously known, but he had succeeded in taking from previous workers what was good and rejecting what was hurtful. He operated by a slight scratch, kept his patients cool, and purged them with calomel and antimony. Thomas Dimsdale was also a most successful inoculator in England at this time, and in 1766 he published an account of Sutton's method. In 1768 he was recommended to Catherine, Empress of Russia, and went to that country to teach the practice to the Russian physicians. He inoculated both the Empress and her son, as well as many of the nobility, and returned to England with the title of Baron and loaded with wealth.

In spite of inoculation and improved treatment, deaths from small-pox increased in numbers. It was calculated that at the beginning of the eighteenth century in London about one in fourteen of the inhabitants died of small-pox, but that during the last thirty years of the century the mortality increased to one in ten. At that time the annual loss of life in Great Britain and Ireland from small-pox was calculated at between thirty-four and thirty-six thousand persons.

The next great advance was marked by the publication in 1784 by Dr. Haygarth, of Chester, of "An Inquiry how to prevent the Small-pox." Haygarth's views were far in advance of his time, and had the real value of them been appreciated there can be little doubt that great benefit would have accrued to the nation. Haygarth took as his starting point the infectious nature of the disease, his words being "that at the present time it is

occasioned by neither climate, soil, nor season, but by infection only." He argued that mankind is not necessarily subject to the disease, and that one, though liable to it, will not take it unless exposed to infection. This infection comes from some person with the disease either directly or through clothing, or something soiled by the discharges or scabs from the infected patient. Although the infection may be carried through the air this is not usual, and it is probable that exposure to fresh air soon renders the poisonous matter inert. Haygarth had not at his disposal facts to prove at what time the variolous patient became infectious, but he shrewdly concluded that such persons were not infectious before the appearance of the eruption—that is, before the third day. He accepts the view that the infecting agent is of the nature of a ferment, "which by an admixture of a few of its particles with the blood occasions the generation of a large quantity of poison." In support of all these propositions he brings forward a number of facts derived from his experience of the epidemic in Chester in the years 1777-1778. In order to apply these theoretical conclusions to practice Haygarth drew up the following rules, and suggested that a reward should be offered to the poor for their observance, while the better classes should be asked to observe them "through motives of humanity, in order to preserve their fellow-creatures from so fatal a pestilence as the natural small-pox." The rules were as follow:—

"I. Suffer no person who has not had the small-pox to come into the infectious house. No visitor who has any communication with persons liable to the distemper should touch or sit down on anything infectious.

"II. No patient after the pocks have appeared must be suffered to go into the street, or other frequented place.

"III. The utmost attention to cleanliness is absolutely

necessary during and after the distemper. No person, clothes, food, furniture, dog, cat, money, medicines, or any other thing that is known or suspected to be daubed with matter, spittle, or other infectious discharges of the patient should go out of the house till they be washed, and till they have been sufficiently exposed to the fresh air. No foul linen, or anything else that can retain the poison, should be folded up and put into drawers, boxes, or be otherwise shut up from the air, but immediately thrown into water and kept there till washed. No attendants should touch what is to go into another family till their hands are washed. When a patient dies of the small-pox particular care should be taken that nothing infectious should be taken out of the house so as to do mischief.

"IV. The patient must not be allowed to approach any person liable to the distemper till every scab is dropt off, till all the clothes, furniture, food, and all other things touched by the patient during the distemper, till the floor of the sick chamber, and till his hair, face, and hands have been carefully washed. After everything has been made perfectly clean, the doors, windows, drawers, boxes, and all other places that can retain infectious air should be kept open till it be cleared out of the house." It was proposed to appoint inspectors, whose duty it would be to see that these regulations were carried out, and also to keep a register of all the cases of the disease with full particulars as to the persons and families infected. Haygarth did not propose to do away with inoculation altogether, but recommended that it should be done generally and at stated periods, say of two years, so that those who did not wish to be inoculated might keep away from those who were, and so escape infection. After Haygarth had elaborated his plan he found that one very

similar in scope and in design had for some time been most successfully adopted in Rhode Island. There small-pox patients were strictly isolated, and if the patient was so ill as not to be able to be moved with safety, the street was boarded up, the fact was advertised in the newspapers, and guards were set to prevent any one coming near the house. So great was the fear of the disease that these regulations, though at first sight irksome, were readily adopted by the people, so much so that it was said that they appeared "more like a popular custom than the restraints of the law." We nowadays can hardly realise the dread with which the deaths from small-pox had inspired the people at the end of the eighteenth century. Looking back after the lapse of years we can appreciate the advance made by Haygarth, and in how much he anticipated the most recent advances of modern sanitary science; but we can also see that his knowledge was insufficient to ensure for his plan more than partial success. The very success, too, which it would have achieved would have led to its ultimate failure. When freedom from epidemic small-pox during a number of years had dulled the fear that the people had of the disease, carelessness of the precautions would in an unprotected community have been followed by further epidemics. It was at this stage that Jenner came and offered a preventive measure against small-pox, which, in spite of the advances of the past century, still remains at once the most complete and most effective preventive measure of dealing with any infectious disease yet known to medical science.

Edward Jenner, a younger son of the Rev. Stephen Jenner, was born on May 17, 1749, at Berkeley, in Gloucestershire, where his father was vicar. The Jenner family had for some time been settled in

Gloucestershire, where the Rev. Stephen was possessed of some landed property. Edward received his early education at Dr. Washburn's Academy in Cirencester, and later became an apprentice to Mr. Daniel Ludlow, a surgeon at Sodbury, near Bristol. Having finished his apprenticeship, Edward went to London to "walk the hospitals," and there became a pupil of the great John Hunter, then one of the surgeons of St. George's Hospital. Jenner appears to have been a favourite pupil of Hunter, and several letters which passed between them at different times have been preserved. In 1772 Jenner returned to Berkeley, where he started in general practice as a physician, and as such seems to have been both successful and popular. He spent his leisure time in carrying out experiments in natural history, collecting material for his old master, John Hunter, and in writing verses. In 1788 a paper by him on the natural history of the cuckoo was read before the Royal Society, and in the following year he was elected a Fellow of that body. He married in 1788 a Miss Catherine Kingscote. In 1792 Jenner's practice had increased so much that he decided to confine himself entirely to medicine, and with that view obtained the degree of Doctor of Physic from St. Andrew's University.

For many years it had been recognised that a certain form of pustular disease of the udders of cows, known as cow-pox, was communicable to the hands of those that milked them, and it was popularly believed by dairy farmers that any one who had once contracted this disease from the cow was afterwards protected from attacks of small-pox either by natural infection or by inoculation. Indeed, in 1774, a farmer named Benjamin Jesty, of Yetminster, had designedly inoculated his wife and two sons with some of the matter taken from sores

on the udder of a cow the subject of cow-pox in order to protect them from an attack of small-pox. This inoculation produced rather severe constitutional symptoms that frightened Jesty at the time, but it was eminently successful, and the sons were found to be immune to inoculated small-pox thirty years afterwards. This tradition had been brought to the notice of Jenner while he was an apprentice at Sodbury, and had attracted him very much. He mentioned the matter to Hunter, and expressed the opinion that use might be made of this method to prevent small-pox. Hunter is said to have given him the advice "don't think, but try." While in practice at Berkeley, Jenner set himself to investigate this tradition and to accumulate facts which would either prove or disprove the validity of the claim set up. The investigation was not an easy one, and though he sought help both from the country people and from his medical friends he found the testimony most conflicting. He recognised, however, that there was more than one disease popularly described as cow-pox, and that there were analogous diseases in both horses and swine. As early as November, 1789, he inoculated his eldest son with swine-pox, and subsequently the child was on three occasions inoculated with variolous matter, but did not develop small-pox. Jenner did not make any secret of his investigations, and we have letters from more than one medical man dealing with the subject. Thus in 1794 Dr. Haygarth, of Chester, wrote to Jenner, giving him advice as to the conduct of his investigations. In May of 1796 cow-pox broke out in one of the dairies near Berkeley, and one of the milkmaids, Sara Nelmes, became infected in her hand. On May the 14th Jenner inoculated some of the matter from the vesicles on this girl's hand into the arm of a healthy boy, James Phipps,

aged eight years. Typical vaccinia followed this inoculation, and subsequent inoculation with variolous matter on two occasions showed that the boy was not susceptible to that disease. From this time till the spring of 1798 Jenner's experiments were interrupted by the disappearance of cow-pox from the neighbourhood. On the 16th of March, 1798, Jenner vaccinated William Summers on the arm with lymph taken from an infected cow. Summers went through an ordinary course of vaccinia, and lymph from him was transferred to one William Pead. Pead developed rather severe constitutional symptoms as a result, but there was no sign of any general eruption on the skin. From Pead several persons were vaccinated, including Jenner's own son, but in his case the vaccination was not successful. Several of these persons were subsequently inoculated with variolous pus, but not one of those so inoculated showed any sign of small-pox. These investigations Jenner now embodied in a paper which he wished to communicate to the Royal Society. This paper never formally came before the Society, but it was submitted unofficially to the President, and was returned to Jenner with the friendly admonition that as he had gained some reputation by his former work it would be unwise for him to present this lest it should injure his already established credit. Jenner was not satisfied with this decision, and in June of the same year he published in book form the paper entitled "An Inquiry into the Causes and Effects of the Variolæ Vaccinæ, a Disease discovered in some of the Western Counties of England, particularly Gloucestershire, and known by the name of Cow-pox." At the time of the publication of this book Jenner went to London, bringing with him a supply of vaccine lymph, and with some of this Mr. Cline, the celebrated surgeon of St.

Thomas's Hospital, vaccinated a child that was under his care in the hospital for disease of the hip. The vaccination was successful, and subsequent inoculation of the child with small-pox virus did not result in the development of that disease. The subject now attracted considerable attention, many experiments were made by different observers, and various papers both for and against the practice were published.

The practice of vaccination was not accepted without considerable opposition, and shortly after the publication of Jenner's book Dr. George Pearson, of London, published a long paper on the history of cow-pox, in which he stated that the disease was often associated with a general pustular eruption. In April of 1799 Jenner published "Further Considerations on the Variolæ Vaccinæ or Cow-pox," in which he maintained his previous position as regards the disease, and stated that he had not met with any cases of general pustular eruption such as Dr. Pearson had mentioned. Jenner suggested that the difference might be due to the fact that his patients were treated in the country, while the others were treated in the town. In May of the same year Dr. Woodville published a paper containing a record of some five hundred cases of vaccination for which the vaccine matter was derived from the cows in a dairy-yard in Gray's Inn Lane. Woodville pointed out that in three or four of his five hundred patients there were very serious symptoms as the result of the vaccination, and one of the children actually died. He concluded "if it be admitted that, at an average, one in five hundred will die of the inoculated cow-pox, I confess I should not be disposed to introduce this disease into the Inoculation Hospital, because out of the last five thousand cases of variolous inoculation the number of deaths has not exceeded the

proportion of one in six hundred." Pearson also recorded similar results, as did various other practitioners. Vaccination in most, if not in all, of these cases was performed with Woodville's lymph. Jenner replied to these strictures in a letter to the *Medical and Physical Journal*, and explained the cause of the trouble as due to the fact that variolous matter "had crept into the constitution with the vaccine." In July, 1800, Woodville replied with another pamphlet, in which he protested as to the purity of his vaccine, and stated that he had lately vaccinated two thousand persons without one alarming symptom. His former ill success he attributed to the vitiated atmosphere of the hospital. At this hospital patients affected with natural and inoculated small-pox were treated with the vaccinated persons. As a result of this controversy more care was taken in the collection of the lymph, and cases with a pustular eruption after vaccination became more scarce. Dr. Moseley, physician to Mr. Fox, was an active opponent of the practice; he published a paper in which he stated that "blindness, lameness, and deformity had been the result of employing the vaccine in innumerable instances." He also suggested that this communication with beasts might "corrupt the mind and excite incongruous passions" in those vaccinated. William Rowley was also a noisy, if not weighty, opponent of vaccination. On one occasion he introduced at his lectures a boy whose face was much swollen and disfigured by an abscess, and thus described the case:— "On this cheek you plainly perceive a protuberance arising like a sprouting horn; another corresponding one will shortly spring up on the other side; for the boy is gradually loosing human lineaments and his countenance is transmuting into the visage of a cow." Mr. John Birch, Surgeon Extraordinary to the Prince of Wales

was also a vigorous opponent, and after his death the following inscription was placed on his tombstone:—"But the practice of Cow-poxing, which first became general in his day, undaunted by the overwhelming influence of power and prejudice, and the voice of nations, he uniformly and until death perseveringly opposed."

That vaccination as performed in the early days of the last century, indiscriminately by all sorts of people and with all sorts of lymph, was sometimes fatal, or followed by serious septic conditions, we can well believe, and that too without any detriment to vaccination; but the occurrence of small-pox after successful vaccination was a real stumbling-block. Just as on the introduction of variolation small-pox was said never to follow it, so on the introduction of vaccination Jenner held that it was an absolute and permanent preventive. Cases, however, soon appeared in which small-pox did follow vaccination. At first such cases were explained by saying that the vaccination was unsatisfactory, or that a spurious cow-pox had been used. In the year 1811, however, the Hon. Robert Grosvenor, who had been vaccinated by Jenner himself ten years previously, was attacked with confluent small-pox. From this attack he recovered, but the case caused a great panic in London, and gave a new impetus to variolation. Statistics of large numbers of cases were then collected, and they showed that though small-pox might follow vaccination, yet it did so less frequently than it followed variolation, and as a rule the disease was of a mild form. In the year 1813 a report was published by the Imperial Institute of France, in which it was stated that of 2,671,662 persons properly vaccinated in France only seven afterwards had taken small-pox. In the Foundling Hospital of London vaccination was introduced in 1801, and though the children were sometimes

intentionally exposed to the infection of small-pox, yet in sixteen years only one slight case of that disease had occurred among them.

Early in May, 1800, vaccination was adopted in the army, and shortly afterwards in the navy, and in July of that year a declaration was signed by many of the chief medical men in London, expressing their confidence in the practice; this document had considerable influence in spreading vaccination. In 1802 Jenner petitioned Parliament for some remuneration as a reward for his discovery, and after an elaborate investigation by a committee £10,000 was voted him by Parliament. In 1806 the continued high rate of mortality from small-pox in London led Parliament to pass a resolution inviting the College of Physicians to consider the progress vaccination had made and the causes that retarded its general adoption. The College of Physicians, having consulted with the sister Colleges of Dublin and Edinburgh, presented a very full report to Parliament in July, 1807. On the receipt of this report the Chancellor of the Exchequer moved that a further grant of £10,000 be given to Jenner. An amendment proposing to increase the grant to £20,000 was eventually carried by a majority of thirteen.

Early in 1800 Dr. George Pearson established in London a Vaccine Pock Institution with the view of investigating the subject and of supplying the world with lymph. To this institution Pearson offered Jenner the position of extra corresponding physician, but as Jenner had not been consulted in the formation of the institution he declined the position. Pearson, at the head of this institution, was looked on as in opposition to Jenner, and in 1803 it was decided to start a Vaccine Society on different principles. The patronage of the Royal Family was obtained, and the Royal Jennerian Society was

started. Moore states that Pearson's Institution was "conducted on so mercenary a plan that it could be of little use." The Royal Jennerian Society, however, did not flourish. The first resident vaccinator appointed was a Quaker, Dr. Walker, who did not get on well with Jenner; religious differences soon occurred, and Dr. Walker was dismissed. A large number of Quaker followers of Dr. Walker left the Society with him and started another called the London Vaccine Institution. This caused a considerable falling off in the funds of the Royal Jennerian Society, and its downfall was completed by the appointment of a young Irish doctor, who appears to have had more aptitude for writing poetry than for vaccinating. A proposal was then made to Parliament to establish a National Vaccine Institute, and after a debate it was decided to do so. In 1808 the National Vaccine Establishment was started under the joint control of the Royal Colleges of Physicians and Surgeons with Jenner as director. In consequence, however, of some difference with the Board Jenner refused to act, and Dr. James Moore was appointed director in his place.

Vaccination was introduced into Dublin in March, 1800, and in 1804 the Cowpock Institution was established under the patronage of the Lord Lieutenant. This institution continues to work at the present time in Upper Sackville Street.

In India there was a good deal of trouble, as in that country the native inoculators made a considerable sum by variolous inoculation, and feared loss of income from the introduction of a practice which they said could not result in good, as no water from the Ganges was mixed with the lymph. The Government, however, offered to pay the native inoculators if they would prac-

tise vaccination instead of variolation, and soon the difficulty vanished.

In Ceylon small-pox committed great ravages, and it was estimated that one-sixth part of the population was killed by the disease. In 1800 the English started small-pox hospitals in which inoculation was practised, yet it was estimated that one in four of those who caught the disease naturally died, and one in thirty of those inoculated. In 1802 vaccination was introduced, the small-pox hospitals were closed, inoculation was forbidden, and in two years it is stated that the disease had disappeared from three of the principal districts of the island.

In 1803 a Spanish physician, Dr. Francisco Xavier Balmis, obtained a commission to propagate vaccination in the American and Asiatic Dominions of Spain, and for this purpose freighted a ship to trade at the ports he touched at. He was singularly successful both in vaccination and trading, so that in two years he succeeded in putting a vaccine girdle round the world and making a considerable fortune.

In January, 1823, Jenner died at Berkeley, having lived long enough to see the practice of vaccination adopted by almost every civilised country of the world, and having received honours and distinctions from many of them.

During the first quarter of the nineteenth century the practice of vaccination spread to a very considerable degree not only in the British Isles but also on the Continents of Europe and America. Thus in 1809-10 we find the State of Massachusetts passing statutes providing for vaccination and for the money to pay for it. In 1801 vaccination was introduced into Sweden, and in 1816 it was made compulsory in that country. In 1825 it was estimated that of the children born 70 per cent.

were vaccinated. In Denmark the practice of vaccination was introduced in 1801, and in 1810 it was made compulsory; from the records of that country it would seem that the law there was very satisfactorily carried out. In England there are no figures to enable one to arrive at a certain knowledge of the number vaccinated at that time; it is, however, probable that the number was considerable, and many of those who were not vaccinated had already had small-pox. In England legislation on the subject of vaccination was much later than in some of the countries of the Continent. In 1802 and 1806 Parliament had made money grants to Jenner as already mentioned, and for some time annual grants were also made to the National Vaccine Establishment. In July, 1840, was passed "An Act to extend the Practice of Vaccination" (3 & 4 Vic., cap. 29). This Act applied to England and Wales, and by a special section was extended to Ireland. It empowered the guardians and overseers of every parish to contract with their medical officers to vaccinate such persons as might apply to them. Payments were to be made to these officers, such payments to depend on the number of persons successfully vaccinated for the first time. By this Act the practice of inoculation was made illegal. In June of the following year, 1841, an Act was passed providing that the expenses for carrying out the Act of the previous year should be charged on the poor rates, but persons vaccinated under the Act were not to be considered as in receipt of parochial relief, and were not to be deprived of any privilege in consequence. In August, 1853, at the instigation of the Epidemiological Society, "An Act to extend and make Compulsory the practice of Vaccination" was passed, but was made to apply only to England and Wales. By this Act children were to be vacci-

nated within three months of birth under a penalty of twenty shillings, and the payment to public vaccinators was regulated. In 1861 a further Act was passed to empower guardians to appoint persons to prosecute offenders under the former Act, and to make parents perpetually liable during the period of default. "The Public Health Act, 1858," was one of considerable importance, as it vested in the Privy Council certain powers for the protection of the public health. The Privy Council was now given powers to issue regulations as to the qualifications of public vaccinators, and generally to control the practice. These powers the Privy Council continued to exercise till they were transferred to the Local Government Board by the "Local Government Board Act, 1871." In 1867 a consolidating statute was passed (30 & 31 Vic., cap. 184), which also introduced some new provisions. The payments to public vaccinators were increased, and the Privy Council was empowered to pay Public Vaccinators additional sums as a reward for successful work. Payment for revaccination was also enforced, and parents were compelled to bring their children to the vaccinator a week after the operation and to permit him to take lymph from them if he wished to do so. This Act was further amended by Acts passed in 1871 and in 1874, but the changes were chiefly of an administrative nature.

In Ireland the legislation has been somewhat different from what it was in England. The Acts of 1840 and 1841 had applied to Ireland as well as to England, but the compulsory Act of 1853 did not apply to Ireland. In 1863 an Act was passed making vaccination compulsory in Ireland (26 & 27 Vic., cap. 52). The children were to be vaccinated within six calendar months of birth, and for the operation a fee of one shilling was to be paid to the

medical officer. The parents were to bring the children to the medical officer for inspection on the eighth day after the operation. In 1868 a short Act (31 & 32 Vic., cap. 87) was passed making inoculation with variolous matter a penal offence, and providing that persons vaccinated by the public vaccinator should not be considered as in receipt of poor law relief. In 1878 a section dealing with vaccination was introduced into the Public Health (Ireland) Act similar to section 31 of the English Act of 1867. In 1879 the last Act dealing with vaccination in Ireland was passed. This Act reduced the time within which a child might be vaccinated from six to three months. It raised the fee for the operator from one to two shillings, and permitted the vaccinator to take, if he wished, lymph from the vaccinated child.

(The Vaccination Acts in force in Ireland are:—21 & 22 Vic., cap. 64; 26 & 27 Vic., cap. 52; 31 & 32 Vic., cap. 87; 42 & 43 Vic., cap. 70. Except sections 1, 2, 3 and 13 of 26 & 27 Vic., cap. 52, which were repealed by section 13 of 42 & 43 Vic., cap. 70).

In Scotland the Vaccination law is practically comprised in one statute that was passed in 1863 (26 & 27 Vic., cap. 108). This Act made vaccination compulsory in Scotland. In that country it would seem that the majority of the people vaccinated are under the care of private practitioners, and the public vaccinators deal only with paupers and the children of paupers, except in the case of defaulters, who must either be vaccinated by the public vaccinator or submit to the penalties of the law. Vaccination in Scotland, too, differs from that in the other parts of the British Isles in that the public vaccinator vaccinates chiefly in the person's own house and not at a dispensary.

In Germany vaccination was early introduced, but for

some time the practice was purely voluntary. In 1834 vaccination was made compulsory for every recruit joining the Prussian army, whether he had been vaccinated before or not. In 1874 compulsion was extended to the civil population. According to this law of April, 1874, every child must be vaccinated before the end of the calendar year following the year of birth, and all school children must be revaccinated in their twelfth year. Obedience to this law is enforced by fine or imprisonment. In Austria there is no compulsory vaccination, though since 1891 vaccination has been "promoted" by the sanitary authorities. Vaccination, though very largely carried out, is not compulsory in France. In Paris when information is received of the occurrence of small-pox in any district vaccinators are sent to the district in question, and they make domiciliary visits for the purpose of offering vaccination to those persons who are unable or unwilling to attend the public vaccination stations. In such cases the vaccination is performed directly from the calf to the arm of the person, and for that purpose the calf is sent to the house from the Institut Vaccinal in a specially constructed van.

The experience gained in the past hundred years of vaccination in different countries has not been unproductive in both our knowledge and practice of the method. When vaccination was introduced by Jenner it was a more or less isolated method of preventive treatment—a substitute for variolous inoculation. Jenner believed that vaccinia was variola so modified by passing through the cow that though it preserved its preventive properties, it had lost its infective properties, and as it had been recognised that one attack of small-pox gave a practical immunity from future attacks of the disease, so he claimed for vaccinia that

it gave practically a perpetual immunity also. The cases in which this immunity failed, when not attributable to inefficient vaccination, he looked on as similar to those exceptional cases in which more than one attack of small-pox occurred. Experience has shown that this position cannot altogether be maintained; primary vaccination confers a certain amount of immunity against small-pox, which immunity, however, lessens as time goes on, and it would seem that the immunity resulting from primary vaccination in infancy is neither so great or so lasting as that resulting from adult vaccination. This decrease in immunity can, however, be renewed by re-vaccination, which should always be done in case of vaccination in infancy, and is advisable even when the primary vaccination took place during adult life. It has also been recognised that the immunity acquired as the result of vaccination and revaccination varies in different individuals. In some it is greater, in others it is less. We are not yet in a position to state the factors on which this difference depends, though it seems probable that it bears some causal relation with the number and extent of the marks resulting from the vaccination. In the time of Jenner this immunity resulting from vaccination was a more or less isolated phenomenon, but recent investigations have given us many parallel examples. In many diseases, the result of infection by micro-organisms, it has been shown that infection by the parasite has resulted in the formation in the body fluids of substances which are antagonistic to the growth of the micro-organism, and not only can the presence of these substances be demonstrated, but their efficiency can also, to a certain extent, be measured. This knowledge has been made use of to a large extent in medical treatment, and the use of these immunising substances, artificially produced, has

been one of the triumphs of modern medicine. Though there is good ground to believe that small-pox is a disease due to the infection of the body by a parasitic micro-organism, and it is probable that this organism has been identified, demonstrative proof of this is not yet forthcoming, and till this proof has been obtained it is not possible to demonstrate the absolute identity of variola and vaccinia, but short of demonstrative proof there seems to be little room for doubt in the matter.

In the practical application of vaccination the advance has been even greater than the advance in our theoretical knowledge. In the early days of vaccination the accidental inoculation of extraneous micro-organisms with the vaccine matter was often the cause of serious results to the vaccinated person. Indeed, the wonder is that this was not always the case; but since aseptic methods have been introduced this accident is both rare and preventable, just as is infection of a wound after a surgical operation. Formerly arm to arm vaccination, or the use of human lymph, was the general rule, and there was always the possibility of the lymph having been taken from a diseased person and so of inoculating the vaccinated person with some other disease as well as with the vaccinia. Now, calf lymph is almost universally employed, and the animal from which the lymph is taken is submitted to a very thorough investigation to show that it is not the subject of any disease before the lymph is inoculated into human beings. Lymph, however, collected under the most favourable conditions is always liable to contamination with extraneous organisms, and its subsequent sterilisation, without destroying its efficacy, was a matter of great difficulty. During the past twenty years this difficulty has been overcome, and now it is possible to remove from the lymph the ex-

traneous organisms without interfering with its activity. This has been effected by the use of glycerine. The fresh lymph is thoroughly mixed with glycerine in a special machine; it is then sealed up in sterile tubes and stored in a dark place for from four to six weeks, after which time it is found to be free from extraneous organisms. At first glycerine was used merely to dilute the lymph and to increase its bulk, for as far back as 1868 Müller had shown that this did not impair the efficacy of the vaccine. The fact that the addition of glycerine was of actual benefit was later pointed out by different workers. The Italians claim that Leoni made this discovery in 1888, his work being published in 1890. The Germans claim priority for Schulz, of Berlin, and state that the method of glycerinating lymph for this purpose has been in use in Germany since 1888. In England the work of Copeman has been of great value. His communication was made to the International Congress of Hygiene in 1891, when he fully demonstrated the value of this process. St. Yves Ménard and Chambon, of Paris, have also done good work in this connection. Much earlier than any of these workers Koch, in 1883, had recognised that extraneous micro-organisms gradually disappeared from lymph to which glycerine had been added, but he does not seem to have recognised the importance of the observations from the vaccinator's point of view. The lymph prepared in this way is carefully examined by bacteriologists before it is issued to the public, and its issue is not permitted unless its purity is beyond suspicion. In these countries every medical student before he becomes a registered practitioner is compelled to take out a special course in vaccination. The rules in connection with this course seem to many to be unduly stringent, but they ensure that every medical prac-

titioner has been instructed in what is believed to be the best and safest way of performing vaccination.

It would not be right to close this review of the history of the prevention of small-pox without paying some tribute to the benefits that have been derived from modern methods of sanitation. The good work that has been done in the past fifty years has borne fruit in the prevention of small-pox epidemics as in the case of the other zymotic diseases. Dealing with small-pox, however, by such methods is of peculiar difficulty. The human race, even when living under the most satisfactory sanitary conditions, appears to be particularly susceptible to the disease, and experience has shown us that very few unprotected persons escape the disease entirely, if they are exposed to infection. Further, the infection seems to be readily carried by the air, and the striking distance seems to be greater than with most other diseases. These difficulties have led to the adoption in many places of special regulations for the prevention of small-pox. In Germany such special regulations do not exist, and small-pox is dealt with under regulations quite similar to those in force for the prevention of other diseases, reliance being placed on the protection afforded to the community by vaccination and revaccination. In England the matter is different, and the following regulations have been issued by the Local Government Board. "Hospitals for small-pox.—In view of the frequently demonstrated liability of small-pox hospitals to disseminate that disease to neighbouring communities, and in order to lessen the risk of such occurrence, the Board require the following conditions to be complied with in the case of small-pox hospitals provided by means of loans sanctioned by them:—1st. The site must not have within a quarter of a mile of it either a hospital, whether

for infectious diseases or not, or a workhouse, asylum, or any similar establishment, or a population of as many as 200 persons. 2nd. The site must not have within half a mile of it a population of as many as 600 persons, whether in one or more institutions, or in dwelling-houses. 3rd. Even where the above conditions are fulfilled, a hospital must not be used at one and the same time for the reception of cases of small-pox and of any other class of disease." In dealing with individual cases of small-pox most health authorities insist on the immediate removal of the patient to an isolation hospital, and at the same time on the isolation of all persons who have recently been in contact with the patient. Sometimes this isolation of contacts is effected by their removal to special places, but in other cases it is considered better to isolate them in their own homes. Whichever course is adopted it is attended with considerable expense. That this expenditure is of benefit, however, is evident from the results which were obtained at Leicester during the epidemic of 1892-1893, and in the last epidemic in Dublin. The efficacy of sanitary measures in preventing the spread of small-pox is also well shown by the case of Australia. Up to 1838 Australia had enjoyed an absolute immunity from small-pox; towards the end of that year the disease appeared in Sydney, but lasted only a short time. It then remained absent from the Continent till 1868, when it was introduced into Melbourne, but was quickly stamped out. In New South Wales, by an Act passed in 1882, notification of small-pox was made compulsory on medical men and householders under heavy penalties. At Sydney notification of small-pox is followed by the compulsory removal of the patient, and all persons who have been in the house with the patient, to the quarantine station at North Head. This station is

670 acres in extent, and situated on the peninsula at the mouth of Sydney Harbour. It is seven miles from the Health Office, with which there is telephonic and telegraphic communication. The persons are conveyed to the station by a steamboat, comfortably fitted expressly for the purpose, and no difficulty has been experienced in effecting the removal. The persons who have been in the house with the patient are detained twenty-one days in quarantine from the date of the last possible contagion. Should a case of small-pox arise among them, those who had been in contact with such infected person would be detained for a further period of twenty-one days, and so on. To facilitate this the exposed persons are distributed in separate groups within the station. They are allowed to receive letters or parcels, and a telegraph operator is employed whose special business is to work the telegraph at their request. Reasonable compensation is given by the Government for loss, and there are heavy penalties whereby the quarantine is secured. Dr. MacLaurin, the President of the New South Wales Board of Health, said that the station is "a pleasant place to stay in, and everything is done that can be done to make the people comfortable; they have nothing whatever to do, and are free from all care, and they can spend the day pleasantly enough; but they do not like it." No one, however, raises any objection to the Sydney system, and "the people are all very sensible about it." In New South Wales there is very little vaccination, and there is no compulsory vaccination law. In all Australian towns a similar system is carried out just as strictly, and on February 5th, 1890, there was not a case of small-pox in all Australia.

There seems to be a tendency in the preventive treatment of small-pox to return to the ancient methods of

Thibet of strict isolation, carried out perhaps with more humanity and with more regard for the interests of the individual than was the case in that country, but still on a similar principle. Where these methods are thoroughly carried out they will probably be found quite efficient, and be beneficial not only in the prevention of small-pox but also in the promotion of the general health of the community. The experience, however, of sanitary workers in these countries does not justify the assumption that they are applicable as yet in their completeness to the conditions of our country. In spite of the great advances in sanitation during the last fifty years it has not been found possible to control the incidence of other zymotic diseases to a similar extent as we have been able to control that of small-pox, though many of these appear to be more amenable to control by sanitary measures than is small-pox. The difference, we believe, is due to the preventive action of vaccination and re-vaccination. We must consequently add our tribute to that of the nations of the world to the illustrious Jenner for his great discovery—a discovery the benefit of which has been and is enormous. Whether Jenner's method will continue to be necessary or not in the future for the prevention of small-pox will depend on whether the great mass of the people will or will not be prepared to submit to the restrictions on their so-called liberty which are entailed by a rigid observance of sanitary laws.

Sir John Moore thought the failure of the present system of dealing with small-pox in the United Kingdom was in the lack of re-vaccination. The example of Germany was an example for the world. Taught by the lesson of the pandemic of 1870–71, the Parliament of the German Empire passed a compulsory vaccination

law in 1874, in accordance with which every child must be vaccinated before reaching the age of one year, and re-vaccinated before twelve years. The result of the system had been shown by the slides. In Germany the hospital treatment of small-pox presented no difficulty: there was no fuel to catch fire when a spark alighted in the centre of one of the German cities. Far different would it be even in well-vaccinated Ireland. Primary vaccination in infancy conferred an absolute immunity for the time being; but whether it was owing to the rapid growth of the body and the multiplication of unprotected cells, that immunity was transitory, and the necessity arose for re-vaccination at an age approaching puberty, when anabolism and katabolism were more nearly balanced. Re-vaccination, he ventured to think, in most cases established a life-long immunity. It was with dismay that one marked the terrible craze of anti-vaccination that was creeping into Ireland. It was a serious matter that in the south-eastern part of Leinster there should be a decided rebellion against vaccination. Dr. Kirkpatrick could render no more valuable service to the country than to take his slides down to Enniscorthy and invite the people to go and see them.

Dr. Donnelly said the Section, and indeed the whole country, owed a great debt of gratitude to Dr. Kirkpatrick. He believed that the present outburst of anti-vaccination was largely due to the fact that nowadays even medical men in Ireland did not see small-pox at all; it was taken for granted that it would not come. At present there are many false statements spread abroad by papers, such as the *Vaccination Enquirer*. About 10 per cent. were unvaccinated, and when the spark came in 1895 small-pox spread among those who were unvaccinated, and also among those who were not re-vaccinated. At the time, certain persons in the hospital to which he was attached refused to be re-vaccinated, and got an attack, but those who were re-vaccinated did not. When the epidemic came there was no trouble in getting the people generally to be re-vaccinated, and the result was that the small-pox was stamped out in a little over a year. Something strong ought to be done to counteract the wave of anti-vaccination, and one of the best things would be to educate the people by having such lectures as we have just listened to delivered in various centres throughout the country.

Dr. E. J. M'Weeney agreed with the suggestions as to the educa-ion of the people. The mere showing of Dr. Kirkpatrick's slides lemonstrating the ghastly thing that confluent small-pox was, nd the marvellous effects of re-vaccination in the German Empire, would carry conviction to every mind. It was one of he great difficulties of the present Governmental system that it vas not possible to bring in any highly competent outsiders nd utilise them for purposes so intimately affecting the public veal. The Local Government Board was keen on having the nti-vaccination delusion stamped out, but its hands were tied, nd it could employ only its own officials. He was responsible or the bacteriological purity of the lymph which was issued to vaccinators in Ireland, and he could testify that it was produced vith the utmost cleanliness and asepticity. Vaccine lymph had very distinct bacteriological flora, which consisted of organisms ound in the skin of the calf. When taken directly from the kin some had a certain pyogenic power, like the staphylococci ound in the human skin; but after subjection to the action of lycerine they seemed to loose that power. The method of vaccinating direct from the calf to the arm had been found to ive too vigorous a local reaction: the micro-organisms had not een attenuated, and the method was very properly not practised n Ireland. He thought Dr. Kirkpatrick might have referred o the services of Dr. Copeman in advocating the glycerination f lymph as a means of getting rid of the organisms or of their ower of producing suppuration. It was an astonishing thing o learn that the infectious nature of small-pox was not recognised ntil 1780.

Dr. Kirkpatrick, in reply, said that although he had not ctually mentioned Dr. Copeman's name, it was recognised that nodern methods of preparing lymph were practically entirely ue to his work, which he had in mind when describing the rocess.

ABSTRACTS.

SECTION OF STATE MEDICINE.

Friday, January 28, 1910.

The President—Dr. E. J. McWeeney, in the Chair.

The Precipitin Test in Medico-legal Work.

The President delivered an inaugural address on this subject. See page 401, *ante*.

Friday, April 15, 1910.

The President in the Chair.

The History of the Prevention of Small-pox.

Dr. Kirkpatrick dealt with the history of the methods that have been from time to time adopted to effect the prevention of this disease. See page 426, *ante*.

A Quinquennial Census.

Dr. W. A. Winter, read a letter from the Society of Medical Officers of Health, who are seeking to have a Quinquennial Census, or enumeration of the population, established. The letter had been referred by the General Council to the consideration of this Section. A resolution was proposed approving of increasing the frequency of the Census, and the Secretary was directed to forward it to the General Council.

SECTION OF ANATOMY AND PHYSIOLOGY.

ABSTRACTS.

SECTION OF ANATOMY AND PHYSIOLOGY.

Sectional Secretary—E. P. M'Loughlin, M.B., R.U.I.

Friday, January 21, 1910.

The President—Professor T. H. Milroy, in the Chair.

The Influence of the Addition of Calcium Salts on the Coagulability of the Blood in vitro.

Professor B. J. Collingwood gave a preliminary communication on the above subject. The coagulation time of blood is largely dependent on the method employed in its withdrawal. For example, the rate at which the blood is shed is a factor of the highest importance. In one individual it was found that when a deep puncture was made and a rapid flow secured the coagulation time was three and a quarter minutes; whilst in the same individual, if a slight puncture only was made and a drop of blood squeezed out, the coagulation time was only twenty seconds. Such an alteration in coagulability as this indicates the necessity of the most careful technique in the investigation of the influence of the addition of calcium salts to blood *in vitro* on its coagulability.

Technique adopted.—Equal bulks of blood were taken from the *same* drop at the *same* moment by two individuals. The following

manipulations were performed at the *same* moments of time by *each* individual, whom I will refer to as *A.* and *B.* :—

1. *A.* mixed his bulk of blood with quarter volume of a given strength of $CaCl_2$ solution. *B.* mixed his bulk of blood with quarter volume of an equi-normal strength of NaCl solution.
2. *A.*, after sealing tube, placed it in incubator at 37° C. *B.* imitated exactly the action of *A.*
3. After a stated interval of time *A.* removed tube from incubator, broke off the end of the tube, and blew out contents on to blotting paper, and noted whether clots were formed or no. *B.* imitated exactly the action of *A.*

The following table gives the results of a series of such experiments :—

Time of Incubation	Same Drops		Same Drops		Same Drops	
	N/20 $CaCl_2$	N/20 NaCl	N/30 $CaCl_2$	N/30 NaCl	N/40 $CaCl_2$	N/40 NaCl
1 min. 30 secs.	—	—	—	—	Nil	Nil
1 ,, 40 ,,	—	—	—	—	Nil	Nil
1 ,, 45 ,,	Nil	Nil	Nil	Nil	Clot	Clot
2 ,, 00 ,,	Nil	Nil	Trace	Trace	—	—
2 ,, 15 ,,	Nil	Clot	Clot	Clot	—	—
2 ,, 30 ,,	Nil	Clot	—	—	—	—
2 ,, 45 ,,	Nil	Clot	—	—	—	—
3 ,, 00 ,,	Clot	Clot	—	—	—	—

Neither in these experiments nor in any other was any evidence obtained of the addition of calcium salts *increasing* the coagulability. It rather appeared that an addition of calcium of such a strength as to appreciably raise the calcium content of the mixture above that of the blood *diminished* coagulability. It was found, however, that this diminution of coagulability was masked

if thrombokinase was present in excess. This is shown by following experiments :—

		Same Drops		Same Drops		
		N/10 $CaCl_2$	N/10 NaCl	N/10 $CaCl_2$ + Thrombokinase		N/10 NaCl + Thrombokinase
Mins.	Secs.			Secs.		
1	45	Nil	Nil	15	Nil	Nil
2	00	Nil	Nil	20	Clot	Clot
2	15	Nil	Trace			
2	30	Nil	+Trace			
2	45	Nil	Clot			
3	00	Nil	Clot			
3	15	Nil	Clot			
3	30	Nil	Clot			

Professor Collingwood concluded by expressing the opinion that the clinical utility of the administration of calcium in the treatment of hæmophilia and allied conditions must depend on other factors than the direct influence of calcium on the blood. He was indebted to the kindness of Sir Almroth Wright for the opportunity to carry out these experiments in the Inoculation Department of St. Mary's Hospital, London.

T. H. Milroy said he thought the difference in coagulability in squeezing the finger was due naturally to tissue contamination. Blood taken off by a cannula would not clot for some time, but if a piece of muscle or tissue was added a clot was got at once. To decide whether calcium chloride in any strength increased coagulability they would have to test with extreme care. In most cases the increase was probably due to vaso-constriction, even in the case of a wound. A point of great importance in the power of lime to increase coagulability depended on the ionisability of the salt. It was quite possible that all the lime did was to aid clotting

by acting as a local constrictor. The cause of thrombokinase hindering it ought to be worked out.

Professor McWeeney said that thrombosis was of frequent occurrence in the state of recovery from typhoid fever. The explanation given was that such patients were always kept on a strictly milk diet, which possessed a considerable content of calcium. Speaking from memory, he thought Wright had reported finding in the blood of such persons four times the normal amount of calcium and twice the coagulability, and he was inclined to attribute it to the exclusive milk diet. In cases of erythema nodosum, Wright claimed that calcium had a good effect, no doubt by producing a general vaso-constriction. With the view that it had some such effect he was inclined to agree from personal experience, as he had known the swellings to disappear after three doses of calcium chloride.

Mr. W. I. de C. Wheeler said it was a general rule to give large doses of calcium for some days before prostatectomy. One might be wrong in such a practice, as an overdose might inhibit coagulation. Was it possible that hæmostasis was due to vaso-constriction when the calcium chloride was administered several days beforehand? It was hardly conceivable that the effect would be maintained up to the day of operation, and therefore cause hæmostasis. Moynihan threw discredit altogether on the administration of calcium, and adopted the method of injecting an alien serum, such as horse serum.

Professor Collingwood, in reply, said he had tried up to N/100. If calcium was added to about the same strength or weaker than the blood calcium, there was no result; if added stronger, there was the tendency to diminish coagulability. So far as he had gone, in no single instance had the addition of calcium increased coagulability. He did not think ionisation of the calcium necessary to the production of coagulation. He did not think Wright had proved his case, as milk contained many things besides calcium. It was said to contain thrombokinase; and the proteid might have something to do with it. In typhoid they had a feeble circulation, which would tend to make the blood coagulate when shed on the surface. As regards continuity of action, calcium differed from other drugs, and was regarded as a physiological stimulant for contraction of the heart. He had done a

number of experiments on himself, and he had not been able to prove that he had absorbed any calcium. He would try taking a pint of milk instead of a drachm of calcium chloride. He had not been able to get any definite results with regard to the excretion in the urine. Injection of serum seemed to be a more rational treatment for the prevention of hæmorrhage than calcium, as the serum contained a fibrin ferment at some stages. A comparison between old and new serum was interesting : serum of four days might not cause coagulation at all, but might be inhibitory. It was a curious fact that men who had given calcium said the bleeding was just as bad as ever, whereas when magnesium was given the results were excellent; and yet one found that magnesium had no power in experiments *in vitro*. He thought the connection must be one of vaso-constriction, and had nothing to do with the direct action of the blood.

Some Observations on the Oxygen Capacity of Blood.

A communication by Dr. J. A. Milroy on the above subject was then read by the President. Haldane's method for the determination of the oxygen capacity of the blood gives excellent practical results ; and, assuming that an oxygen capacity of 18.5 cc. corresponds to blood having the normal percentage of hæmoglobin, the results obtained have been successfully used in standardising solutions for his colorimetric method for the estimation of hæmoglobin. Yet the exact relation between the oxygen capacity of a given sample of blood and the percentage of hæmoglobin in it is still uncertain. According to Hüfner one gramme of oxyhæmoglobin is capable of yielding 1.34 cc. of oxygen, either *in vacuo* or as a result of the action of potassium ferricyanide. The same figures apply to carboxyhæmoglobin. Assuming that 18.5 cc. is the oxygen capacity of normal blood, the percentage of hæmoglobin present in it would therefore be 13.8. Haldane, Bohr and his pupils dispute Hüfner's results. As the result of a large number of determinations, Bohr states that the quantity of oxygen fixed by one gramme of hæmoglobin is not constant. The chief reasons for this discrepancy are the following :—First, oxyhæmoglobin, during the process of purification by crystallisation, becomes partly converted into methæmoglobin. The latter pigment does not yield up its

oxygen either *in vacuo* or when treated with potassium ferricyanide. Another source of error lies in the fact that the oxygen capacity of the hæmoglobin derived from different animals appears to differ in value. For these reasons it is of considerable importance to determine the oxygen capacity of the blood in relation to some more definite standard than hæmoglobin. The power possessed by hæmoglobin of fixing oxygen and carbonic oxide has been shown by Hoppe-Seyler to be entirely dependent on the hæmatin radicle. He also found that the quantity of carbonic oxide fixed by it corresponded to one molecule of carbonic oxide for each atom of iron. This carbonic oxide capacity is equivalent to 399.8 cc. for each gramme of iron. Pregl and Hüfner, with the aid of more accurate methods, proved that in alkaline solution reduced hæmatin unites with the exact theoretical proportions of carbon monoxide, forecast by Hoppe-Seyler's results. Since hæmatin has a constant composition whatever the source of the blood from which it is derived, and since the quantity of iron in it is accurately known, it follows that the oxygen capacity of blood is best stated in relation to the quantity of iron rather than in terms of hæmoglobin. Bohr carried out a large number of concurrent estimations of the iron in blood and the quantity of oxygen obtainable *in vacuo*, and introduced the phrase specific oxygen capacity of blood to define the quantity of oxygen yielded by a quantity of oxyhæmoglobin containing one gramme of iron. The values which he obtained were, as a rule, considerably lower than the theoretical one of 399.8 cc. He obtained similar results for the carbonic oxide capacity of blood. In some experiments on the specific carbonic oxide capacity of pure reduced acid hæmatin, Dr. Milroy also obtained results distinctly lower than the theoretical, but closely approximating to those of Bohr. Notwithstanding these facts, the specific carbonic oxide capacity of reduced alkaline hæmatin is almost certainly constant, since the results obtained by three independent observers—Hoppe-Seyler, Hüfner and Pregl—agree very closely with one another. Further light on the subject would probably be obtained by carrying out concurrent estimations—first, of the iron in different samples of blood; secondly, of the oxygen capacity by the ferri-cyanide method; and thirdly, of the carbonic oxide capacity of blood in which the hæmoglobin had been decomposed

so as to yield reduced alkali hæmatin. In view of Hoppe-Seyler's, Hüfner's and Pregl's results, the first and second series of estimations would be most likely to yield concordant results. Dr. Milroy was at present engaged on work along these lines.

Estimation of β-hydroxylbutyric Acid.

Mr. W. M. Caldwell gave a preliminary communication on the method of estimation of β-hydroxylbutyric acid, which was discussed by the President and Professor McWeeney.

Unusual Vermiform Appendix.

Mr. W. I. de C. Wheeler exhibited an unusual type of vermiform appendix got in the dissecting room about a week previously. The specimen reached almost the maximum length of six inches. It lay close to outer side of the posterior wall of the ascending colon, outside the peritoneal cavity. At the end of the appendix was a long fibrous cord, continuous with the peritoneum, over the front of the surface of the kidney. The condition might be explained by an intrauterine peritonitis, the tip of the appendix being anchored above by the inflammatory material. The cæcum was of the conical fœtal type in the same subject. The colon, instead of passing from the left iliac fossa into the pelvis, turned sharply upwards and formed a long loop in the abdominal cavity before finally descending into the pelvis to end in the rectum. There was marked narrowing at the upward bend, but nothing pathological. The colon was laden with fæces, and Mr. Wheeler imagined that the patient died of intestinal obstruction.

Model of Chondrocranium of Fœtal Trout.

Dr. N. C. Rutherford showed a model of the brain, chondrocranium, sense organs and cranial nerves of *Trutta Fario*. The model was constructed of wax plates on the Born method at a magnification of one hundred times. The period illustrated was an embryonic one at the beginning of the week before hatching. At this period, the development of both brain and chondrocranium illustrate a transition stage of considerable value with reference to the method of their evolution—a value still further enhanced by the inclusion in the model of the particular features of the cephalic sense organs. The various portions of the brain were demon-

strated, and the relative stages of growth noted. The cranial nerves were touched upon with especial reference to the dromic sense organs of the head and lateral line. The cartilaginous elements of the cranium were detailed according as they formed part of the viscerocranium and neurocranium, and of the divisions of the latter—the palæocranium and neocranium. The model was much admired by the meeting, and several members expressed their appreciation of this beautiful exhibit and of the interesting demonstration given by Dr. Rutherford.

Friday, April 29, 1910.

The President in the Chair.

Development of Teeth in Primates.

Professor Symington, F.R.S., exhibited a number of skiagrams illustrating the development of the teeth in primates, and also the relation of the teeth to the maxillary antrum.

Professor M'Loughlin asked if Professor Symington had made any observations as to the time of eruption of the permanent canine teeth in the male anthropoids, and the relation of that time to the time of eruption of the third permanent molars.

Professor Symington, in reply, said that some authors state that the canine tooth, especially in the gibbon, was very late in erupting. He had no suitable material for observation in the older stages, as apes in the Zoological Gardens died young, and there were certain objections to the use of the ordinary dried specimens. The teeth shook about in the cavities, and became readily displaced. Museum curators did not like skulls divided in the middle line, and satisfactory skiagrams could not be got with the entire skull. It was not easy to get a moist specimen of the older stages of the apes, but he hoped to get a complete series before long.

A Note on Superfœtation.

Dr. Cassidy showed a specimen of the above taken from a cat. It showed in left horn of uterus a well-developed embryo five

inches long and six or seven weeks old. It possessed a fully formed placenta, umbilical cord four inches long, and membranes complete. Left horn contained a much smaller fœtus two inches long, which was enveloped in its own bag of membranes. The uterine body was one inch long, half an inch broad, one-sixteenth inch in thickness. A septum divided it into two parts, extending to external os. Decidua lined both horns. The right ovary showed a well-marked true corpus luteum, which was obviously of recent date, and accounted for the second embryo. The left ovary had a true corpus luteum, which was distinctly older than the one on the right side. The interest of the specimen lies in the fact that there were two embryos, one occupying each horn, and showing a great difference in period of growth. The smaller by no means represented a "blighted ovum," as both microscopically and on section it appeared quite normal. Before arriving at the conclusion that the specimen was a case of superfœtation, two essentials had to be satisfied—(1) Did ovulation and œstrum recur after first fecundation? (2) Was there, anatomically speaking, a clear passage for a second male element at a later period? The recent appearance of the true corpus luteum in the right ovary appears sufficient justification for the recurrence of second œstral period. The abdominal ostium of the right tube was still patent, and the passage from the external os was clear, owing to the presence of the septum and absence of decidua in the right half of the uterine body. Superfœtation is of not infrequent occurrence amongst mammals. Kroon (Geburtshilfe Kleineren Haustieren) relates a case of a goat bred September 14, 1897, œstrum and copulation recurring fifty-two days later. One hundred and fifty-two days later she expelled three fully formed embryos, two living and one dead. Next day, when membranes were expelled, three more perfectly formed, but imperfectly developed, embryos were discovered. Tapken, quoted by De Bruin, gives a case of superfœtation in a sow which was bred on February 22, 1890. Œstrum and copulation occurred fourteen days later. In the following June she gave birth to seven live pigs, and fourteen days later to nine live and three dead pigs. Reasoning from these cases in mammals where a double uterus exists, and that, exceptionally, the ordinary physiological rule of ovulation being in abeyance during

pregnancy is dispensed with, superfœtation seems not alone possible, but extremely probable. Whether this applies also to human gestation is extremely doubtful. In the thirty-eight recorded cases of bicornuate uteri in women there is no case mentioned of superfœtation having occurred. Obstetrical literature contains several recorded cases of what presumably were instances of superfœtation, but whether true superfœtation occurs in the normal human uterus is very doubtful. With the thickened decidua blocking the lumen of tube and the plug of mucus which blocks the cervical canal at an early date, the passage of another spermatozoön at later date is rendered very improbable.

Professor Mettam said he did not think it exceptional to find cases of that form of gestation, seeing that the animals had a bicornuate uterus. He had convinced himself that ovulation occurred during pregnancy, and that was probably the explanation of animals going a long time in pregnancy.

Professor Thompson said it did not seem beyond the bounds of possibility that the two were impregnated at the same time, and that for some reason one developed and the other did not. They saw the same thing in the seeds of plants.

Dr. Cassidy, in reply, said that, in looking for an explanation for these cases, we must ask ourselves the question—Was it possible for a spermatozoön to live in the passages for four weeks and then fertilize another ripe ovum ?

Complete Transposition of the Viscera.

Mr. Seton Pringle read a note on a case of complete transposition of the viscera. The condition was found in a man, aged twenty-seven, who was the subject of extensive cancer of the rectum. He opened the abdomen for the purpose of fully examining the extent of the disease, and, if removal was out of the question, of performing a colotomy. He found a transposition of all the abdominal organs, and as removal was impossible, did a colotomy, fixing a knuckle of iliac colon in an incision in the right inguinal region. The heart was also situated in the right thorax, the apex beat being in the fifth right intercostal space, one and three quarter inches inside the nipple line. He, in referring to the theory associating these cases of

transposition with twin birth, pointed out that his patient said he was not one of twins, nor was there any record of a fœtus papyraceus.

Professor Dixon said that, in twin formation, an early separation in the embryonic plate, so that the resulting twin embryos would be ventrally opposed, was held to afford an explanation of transposition of viscera in one of the embryos.

Professor M'Loughlin favoured the view that "transposition" was dependent on a primitive vascular irregularity of the abnormal embryo. The direction of the first bend of the tubular heart was probably determined by the internal pressure on the wall of the vessel being greater on that side owing to the greater rush of blood from the left omphalo-mesenteric vein, which is larger than the right. An accidental cause leading to the production of a larger right vein would produce a primary bend of the heart to the left and so initiate the changes responsible for transposition of viscera in the thorax. Similarily a venous irregularity might be responsible for transposition of the abdominal viscera independently of the thoracic.

Dr. Rutherford said he thought it should be remembered that the opposite umbilical vein, even although at a slightly later period, enlarged considerably as compared with its fellow, so that it was, perhaps, going rather far to attribute the position of the heart to the influence of the blood flow through one of the veins bringing blood to it. Though the first bend of the heart was to the right, the great growth was afterwards to the left. It was interesting that in the development of the teleostean heart they had an early bend to the right, and that, despite the fact that the pericardial cavity was very much larger than the contents of the cavity.

Professor Geddes showed specimens and lantern slides of developmental and acquired abnormalities met with in the dissecting room during the winter session 1909–10.

The first of these was an *abnormal nasal duct*, which opened into the middle instead of into the inferior meatus of the nose. On dissection it was found that there was a thin fibrous cord occupying the normal position of the duct. No record of a similar abnormality had been discovered in the literature.

The next specimens shown were *abnormal bicuspid teeth*.

One of these was a second lower bicuspid of the right side, which possessed two distinct fangs, of which one was partially subdivided so that the tooth showed indications of possessing three roots.

An *abnormal right lung* was next demonstrated. In it the oblique fissure was well formed, but there was merely an indication of the horizontal fissure. In addition there were two irregular fissures in the region of the apex, and a cardiac lobe was present. On dissection it was found that though the horizontal fissure was superficially absent, there was no structural reason, so far as the arrangement of the bronchi was concerned, why it should not have been present. The irregular fissuring at the apex was interpreted as representing an attempted development of two apices. This was deduced from the fact that there seemed to be two distinct apical bronchi. The condition of the lung in the seal, and the occasional condition of the lung in the human being, in which the vena azygos major lies in a deep groove separating what might be termed an internal apex from an external, was referred to. In this case the vena azygos major had been in its usual position.

Several *abnormal livers* were then demonstrated. One of these possessed no quadrate lobe, the ligamentum teres passing backwards immediately superior to the gall bladder. In consideration of the arrangement of the fissures on the inferior surface of the liver, it was suggested, as a possible explanation, that the umbilical vein had in this case developed from the vein of the right side.

A cast of a *peculiar rectum* was then demonstrated. In this case the pelvic colon passed down the left side of the pelvic cavity, and after forming a U-shaped bend entered a greatly dilated rectum from below.

At the close of his demonstration Professor Geddes showed an *unusual cæcum and appendix*, and a series of photographs to illustrate the mechanical effects of a massive pleural effusion.

Professor Symington said Professor Geddes, in exhibiting the abnormal nasal duct, had mentioned that in certain animals it opened into the cavity by several orifices, and he would like to hear if Professor Geddes knew any in which it opened above the inferior turbinal. [Professor Geddes: No.] The

literature of abnormalities was so enormous that it was difficult to be certain; but it was a very unusual condition. With reference to the pleural effusion, he thought there was a very serious defect in the conducting of *post-mortem* examinations. It seemed to him that a great deal could be learned if some hardening fluid was injected and the organs were fixed in position. The object of the pathologist was to get the organs out of the body, and there was little attention paid to the topography of the body under pathological conditions. He took it that the grooves shown in the liver were due to some irregularity in the contraction of the diaphragm, which was always found lying in the grooves. With regard to distended rectum, in some cases the folds in the rectum practically disappeared in the distention.

Dr. Moorhead asked if Professor Geddes had had an opportunity of observing the position of the double fold of the pleura, which extended from the back of the heart to the vertical column. About a year ago one of the Keiths described a case occurring on the right side and displacing the pleural fold to the left side, and he regarded it as proving that the angle of dulness was due to actual displacement of the fold. Extreme difficulty was found in carrying out Professor Symington's suggestion with regard to *post-mortems*, which were generally done in a hurry, with friends waiting outside the door to inter the remains. As far as possible they aimed at letting the patient die with the fluid removed. When they did make use of a hardening agent, it was found to defeat one of the main objects of a *post-mortem*—the finding of the cause of the pathological condition. He would like to know if Professor Geddes found any cirrhosis in the liver which he described as having an extra lobe. He often saw the thin edge described, and he generally found it associated with extreme atrophy or cirrhosis.

Professor Thompson asked for particulars as to the age and sex of the cases of abnormal rectum, and whether there was evidence that the rectum had been used as a permanent receptacle instead of an evacuating mechanism.

Professor Dixon said they must all have been struck with the extreme constancy of the gall-bladder, though there did not seem to be any reason why it should always be in the position in which it was found. He only remembered one case in which

it varied. He would be inclined to think, however, that, in Professor Geddes' case, it was the gall-bladder that was astray and not the vein. He would like to be convinced that the remarkable specimen of abnormal nasal duct was not caused by some surgical interference. The duct might have become blocked at some time, and an instrument might have gone astray and opened through the loose bone and tissue into the middle meatus.

Dr. Pearson said he had seen several gall-bladder operations in which the gall-bladder did not appear to be in its usual position, and was found in the middle line.

Professor M'Loughlin asked if the root of the bicuspid tooth which was showing signs of division was the posterior root. If so, it would conform to the broad posterior root of the lower bicuspids of the anthropoids. [Professor Geddes : It was the posterior.] He would like to know if the so-called accessory lobe of the liver was a normal piece of liver, and if its ducts were traced.

Professor Geddes, in reply, said he did not see any difficulty in believing that the abnormal duct was a developmental error. The matter was still under investigation, and microscopic slides would be made. The evidence in the case of the gall-bladder suggested the probability of its being a vascular error, and not a developmental error on the part of the gall-bladder. With regard to the question as to how the pleura was disposed, there was no doubt that the right pleural sac was very large posteriorly. One was hardly justified in suggesting that that was necessarily due to massive effusion, since they met the condition so frequently in cases where there was no effusion. The majority of the really large rectums were found in females, and there was usually a very large fæcal load when the subject came in. But they had occurred also in men, and sometimes in young men.

Human Embryo.

Dr. N. C. Rutherford showed lantern slides of a human embryo of the fifth week. The first of these showed the opened chorionic vesicle, the embryo being enclosed in the amnion, while the yolk sac was still attached and lay in close proximity. The second showed the amnion opened, and a very well-preserved embryo lying inside. Other views of the embryo separated from

the appendages were shown in conjunction with a millimetre scale which indicated its length to be almost exactly eight millimetres. The photographs were entirely untouched or retouched. The characteristics of this stage of development of the external form were clearly demonstrated by the various specimens. Such features as the protovertebral markings, the closing cervical sinus, the state of development and rotation of the limbs, the commencing appearance of the middle digit on the left side indicated the age of the specimen as almost exactly the middle of the fifth week. He was of opinion that the embryo had no exact counterpart among those collected by Keibel and Hochstetter. It was intermediate between Figs. XVIII. and XIX. of Keibel's Normentafeln and between Hochstetter's Embryones Humani Re III. and Ma I. That being the case, Dr. Rutherford expected that further examination of the specimen by serial section and reconstruction would be productive of some profitable results.

Human Embryo showing Ectopia Cordis.

Dr. Rutherford further exhibited a lantern slide of the external form of a human embryo, showing ectopia cordis. He stated that, as was to be expected in such a pathological condition, the features of the external form were somewhat anomalous, and did not allow of any exact estimate of the age of the specimen. Judging, however, by the state of development of the limbs, he would place it early in the fifth week. The entire absence of an anterior wall to the pericardium was very plainly to be seen, the separate chambers of the heart being clearly visible on the surface. The thoracic defect reached forwards to the root of the neck, but fell short of the umbilicus behind. He attributed the condition to the failure of growth of the ventro-lateral process which springs from the protovertebræ, resulting in a defect analogous to that of spina bifida.

Professor Dixon asked if Dr. Rutherford had noticed little nodules in the amnion a short distance away from the embryo. They seemed to be common in the Rotunda cases, and it was thought that they were associated with syphilitic subjects.

Professor M'Loughlin said that spina bifida had been produced experimentally by treating the eggs of tadpoles with a

weak sodium-chloride solution. It was, consequently, suggested that some unusual chemical condition or irregularity of the blood of the mother was responsible for the condition of ectopia cordis. The condition of spina bifida had also been produced in a mechanical way by shaking. Was there a condition of the womb of the human mother that might be regarded as a parallel? It was not unlikely that a womb in an unhealthy condition would not allow the ovum to nest properly in the decidua. Improper nesting would lead to improper development of the chorion. This would lead to improper nourishment of the embryo, which would be the same as treating it with an abnormal fluid. Might not an endometric condition of the womb be a cause of such abnormalities?

Dr. Rutherford, in reply, said the question of the development of eggs in the unsuitable media was a very complicated matter, and the few facts known were insufficient to give a basis for any clear hypothesis. As far as he remembered, there was nothing abnormal in the amnion.

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